MEMOIRS OF THE MANCHESTER
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NOTE.

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MEMOIRS

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I. *On the Internal Cohesion of Liquids and the Suspension of a Column of Mercury to a height more than double that of the Barometer.* By Prof. OSBORNE REYNOLDS, F.R.S.

Read April 16th, 1878.

Introduction.

THE ease with which, under ordinary circumstances, the different portions of liquid may be separated, is a fact of such general observation that the inability of liquids like water to offer any considerable resistance to rupture appears to have been tacitly accepted as an axiom. In no work on Hydrostatics does it appear that the possibility of water existing in a state of tension is so much as considered; and suction is always described as being solely attributable to the pressure of the atmosphere.

The limit, of 32 feet or thereabouts, to the height to which water can be raised by suction in the common pump, and the sinking of the mercury in the barometer-tube (leaving the Torricellian vacuum above) until the column is at most only 31 inches (sufficient to balance the highest pressure of the atmosphere), are phenomena so well known as to be almost household words with us. It is not, therefore, without some fear of encountering simple incredulity that I venture to state

The Object of this Communication.

In the first place my purpose is to show that certain facts, already fully established, afford grounds for believing that almost all liquids, and particularly mercury and water, are capable of offering resistance to rupture commensurate with the resistance offered by solid materials. In the second place, I have to describe certain experimental results which, as far as they go, completely verify these conclusions and subvert the general ideas previously mentioned as to the limits to the height to which mercury can be suspended in a tube, or water raised by suction. And, in conclusion, I shall endeavour to explain the nature of the circumstances which have resulted in the practical limits to these phenomena.

The Separation of Liquids is not caused by Rupture.

Although the smallness of the force generally requisite to separate a mass of liquid into parts leads to the supposition that the parts of the liquid have but little coherence, it may be seen on close examination that this supposition is not altogether legitimate; for such separation of a liquid as we ordinarily observe takes place at the surface of the liquid, is caused by an indentation or running-in o the surface, and not by an internal rupture or simultaneous

separation over any considerable area. Thus when we see a stream of liquid break up to drops, the drops separate gradually by the contraction of the necks joining them, as shown in fig. 1, and not suddenly as in fig. 2. And the ease with which portions of a liquid may be separated by the forcing or drawing in of the surface affords no ground

Fig. 1.

Fig. 2.

for assuming that the liquid is without coherence, any more than does the ease with which we may cut a piece of string, cloth, or metal with sharp shears, or even tear some of these bodies by beginning at an edge, prove that they are without strength to resist great force when these are applied uniformly so as to call forth the resistance of all the parts of the body simultaneously. It is true that under certain circumstances we observe the internal rupture of liquid—whenever bubbles are formed, as when water is boiled; but under these circumstances we have no means of estimating the forces which cause the internal rupture: they are molecular in their action; and, for all we know, they may be very considerable. Having thus pointed out that the ease of separation of the parts of a mass of liquid does not even imply a want of cohesion on the part of the liquid, I shall now point out that we have in common phenomena

Evidence of Considerable Cohesion.

These are, for the most part, what are considered minor phenomena; they are confined to the surface of the liquid, and are included under what is called "capillarity," or "surface-tension."

The phenomena of capillarity or surface-tension have recently attracted a great deal of attention; and many important facts concerning them have been clearly elucidated, some of which bear directly on my present subject.

Of the phenomena I may instance the suspension of drops of water, the rising of water up small tubes, the tendency of bubbles to contract, and the spherical form assumed by small fragments of mercury.

These phenomena and others are found to be explained by the fact that the surface of these liquids is always under a slight but constant tension, as if enclosed in a thin elastic membrane.

No satisfactory explanation as to the cause of this surface-tension has, I believe, been as yet found; but the fact itself is proved beyond all question. It is a molecular phenomenon; and in order to offer any explanation as to its cause, it would be necessary to adopt some hypothesis respecting the molecular constitution of the liquid. Such an explanation making the surface-tension to arise from the cohesion of the molecules of the liquid is, I believe, possible; but this is beside my present purpose, which will be completely served by showing that

The Surface-tension proves the existence of Cohesion.

To prove this requires no molecular hypothesis; but, before proceeding, it may be well to define clearly the term cohesion.

Cohesion in a liquid is here to be understood as a

property which enables the fluid to resist any tendency to cause internal separation of its parts—any tendency to draw it asunder; or, more definitely, it is the property which enables a liquid to resist a tension or negative pressure.

Let us suppose a mass of liquid without internal cohesion. Then any external action tending to enlarge the capacity within the bounding surface of the liquid would at once cause the interior of the liquid to open, and a hollow would be formed within the liquid without any resistance on the part of the liquid. Such a condition is inconsistent with surface-tension; for the tension of the surface of the internal hollow would tend to contract the hollow; and since the interior of the hollow is supposed to be empty, there could be no resistance to the tendency of the surface to contract, such as that offered by the pressure of the gas within an ordinary bubble. Hence any force that might, under the circumstances, balance the surface-tension and keep open the hollow must be supplied by the suction or cohesion of the liquid outside.—Q. E. D.

Again, *the intensity of the cohesion is determined by the intensity of the surface-tension and the smallness of the least possible opening over the surface of which tension exists.*

So far as has yet been determined by experiment, it has been found that the surface-tension is independent of the curvature of the surface—is constant for the same liquid. Assuming that this is the case, it follows that the intensity of the force necessary to keep a spherical bubble or opening from contracting (whether this force arises from the pressure of the gas within the bubble or the cohesive traction of the liquid without the opening) is equal to twice the intensity of the surface-tension divided by the radius of the sphere. *Hence the cohesive tension must be equal to twice the surface-tension of the liquid divided by the diame-*

ter of the smallest opening for which the surface-tension exists.—Q. E. D.

It immediately follows from the foregoing proposition, that no matter how small the surface-tension may be, if it is finite even when the opening is infinitely small, then the cohesion of the liquid must be infinitely great. For, if the liquid were continuous in its origin, the opening must always be infinitely small; and hence to cause such an opening would require infinite tension.

That the cohesion is infinitely great is not probable, to say the least. Hence it is improbable that the surface-tension remains finite when the opening becomes infinitely small. As has already been stated, it has been found that the surface-tension is constant, or nearly so, under ordinary circumstances; but it has never been measured for bubbles of very small diameter, and there appears to be every probability that, when the size of the bubble comes to be of the same order of small quantity as the dimensions of a molecule, the surface-tension must diminish rapidly with the size of the bubble.

If this is the case, then we have a limit to the cohesion, although it is probably very great for most liquids, something like the cohesion of solid matter of the same kind. That is to say, it is probable that it would require nearly as great intensity of stress to rupture fluid as it would to rupture solid mercury, or as great tension to rupture water as to rupture ice.

The Effect of Vapour.

Nothing has yet been said about the effect of the pressure of vapour within the bubbles in balancing the surface-tension. It may, however, be shown that this can be of no moment. Even supposing that the tension of the vapour within the opening of the liquid were equal to the

tension due to the temperature under ordinary circumstances, this would be inappreciable. So that, unless the tension of vapour within small openings were much greater than that in larger openings for the same temperature, its effect might be neglected; and so far from this being the case, Sir William Thomson has shown that the pressure of the vapour within a bubble at any particular temperature diminishes with the size of the opening. Hence it is clear that this vapour can have no effect on the result—a conclusion verified by the now well-known fact that water may be raised to a temperature high above 212° without passing into steam.

Experimental Verification necessary.

This line of reasoning has been apparent to me now for several years. I find notes on some of the principal points which I made in 1873; and for several years I have pointed out the conclusions arrived at as regards the probable cohesion of water to the students in the engineering class at Owens College. I have, however, hitherto refrained from publishing my views, because I had no definite experimental results to appeal to in confirmation of them. Experimental indications of such a cohesive force were not wanting, but they were not definite. And although methods of making definite experiments have often occupied my thoughts, certain difficulties, which turn out to have been somewhat imaginary, kept me from trying the experiments.

It had always appeared to me that, in order to subject the interior of a liquid mass to tension, it would be necessary to, as it were, hold the surface of the liquid at all points to prevent its contracting. To accomplish this, it was necessary to have the liquid in a vessel, to the surface of which the liquid would adhere as water adheres to glass.

The experiment which I had conceived would have been equivalent to a vertical glass tube more than 32 feet long, closed at the upper end and open at the lower, so that when the tube was full of water the column would be higher than the pressure of the atmosphere would maintain, and hence could only be maintained by the cohesion of the water. The difficulty of such an experiment, however, appeared to be great. It was clear that if mercury could be substituted for water this difficulty would be much reduced; but then mercury does not readily adhere to glass, and the ordinary method of making barometers seemed to disprove the possibility of making it adhere.

It was only on the 2nd of this month that an accidental phenomenon at once afforded me the experimental proof for which I had been looking.

First Experiments.

The phenomenon was observed in a mercurial vacuum-gauge (a siphon gauge which admitted a column of mercury 31 inches long). Before the mercury was introduced the tube had been wetted with sulphuric acid, a few drops of which covered the mercury on both ends of the column.

The gauge had been in constant use as a vacuum-gauge for three weeks; and, probably owing to the action of the acid on the mercury, a little gas had been generated between the mercury and the closed end of the tube, sufficient to cause the column to sink to $27\frac{1}{2}$ when the barometer stood at 29. To get rid of this air, the tube was removed from its situation and placed in such a position that the bubble of air passed along the tube and escaped, the open end of the tube being entirely free. Before the tube was tilted in this way, the unbalanced column was $27\frac{1}{2}$ inches long. When tilted, the mercury ran back right up to the end of the tube as the bubble of air passed out. On erecting the tube

again, the mercury remained up to the end of the tube, except about one eighth of an inch, which was filled with sulphuric acid. The unbalanced column of mercury was therefore 31 inches long. At first the full significance of this phenomenon was not recognized; but in order to ascertain that the tube was cleared of air, it was moved gently up and down to see if the mercury clicked, as it usually does when the tube is free from air, but the mercury did not move in the tube. The rapidity of the oscillation was thereupon increased until it became a violent shake, and, as the mercury still remained firm, it was clear that some very powerful force was holding it in its place. The tube being in a vertical position, was then left in order that the barometer might be consulted. This was standing at 29 inches. After a few seconds, when the gauge was again examined, the column no longer reached the end of the tube, but stood at 29 inches. As it was singular that the mercury should have quietly settled down after having resisted such violent shaking, the tube was again inclined until the mercury and acid came, apparently, up to the end of the tube; but this time on the erection of the tube the mercury at once settled down. That is to say, it settled down gradually as the tube was erected. At first what appeared to be a very small bubble opened in the sulphuric acid; and this enlarged as the top of the tube was raised. On again inclining the tube until it was horizontal, and examining it closely, a minute bubble could be seen in the acid, and it was this bubble which expanded as the tube was erected, and so allowed the mercury to descend. To get rid of this bubble, the tube was turned down so as to allow the bubble to pass along the tube; but, owing to its small size, it did not pass many inches along the tube before it became fixed between the mercury and the glass. When the bubble came to a standstill at about six inches from the

end of the tube, the gauge was again erected; the bubble immediately began to move back, but so slowly that it was some seconds before it entered the region of no pressure. During this interval the mercury remained up to the end of the tube; but the bubble, as soon it neared the top of the tube, expanded and rapidly rose to the top of the tube, leaving the column at 29 inches. This operation having been repeated several times, it became quite evident that it was this small bubble which, either by rising up the tube or being generated at the top, had caused the mercury in the first instance to sink. As the bubble would not pass out by itself, the tube was tilted so as to allow a larger bubble of air to enter; and having been left standing for about twelve hours to allow the small bubble to unite with the larger one, it was again tilted so as to allow the air to pass out. When this was done the mercury again remained firmly against the end of the tube and did not descend when violently shaken. The open end of the tube was then connected with an air-pump and exhausted until the pressure within it fell to about four inches of mercury. This operation occupied some seconds; but all this time the mercury did not move from the end of the tube; but eventually the column opened near the bottom of the tube and a large bubble appeared, which rose up the tube, the the mercury falling past the opening. That the breaking of the column so near the bottom of the tube was owing to the presence at that point of a small bubble of air was almost proved by the fact that, on readmitting the air to the open end of the tube and inclining the tube to see if it was free from air, there was found a minute bubble which played exactly the same part as the small bubble which had been previously examined.

At the instant previous to the rupture of the column at the bottom of the tube, there must at the top of the tube

have been an unbalanced tension or negative pressure equal to 27 inches of mercury; and this tension did not break the continuity of the column. Hence I had a proof that the cohesion within the mercury and the sulphuric acid as well as the adhesion of the sulphuric acid to the mercury and the glass is sufficient to resist this very considerable tension.

Further Experiments.

In the hope of improving the experiments, another gauge was constructed, the tube being $\frac{5}{16}$ of an inch in internal diameter and 35 inches high. Into this tube mercury and sulphuric acid were introduced, as in the first tube. But on trying to get rid of the small bubbles of air, it was found impossible to do so, as bubbles were continually generated. Hence it appeared that the three weeks during which the mercury and sulphuric acid in the first tube had remained in contact had had an important influence on the result. Failing in this attempt, it occurred to me to try if water would answer the purpose as well as sulphuric acid. Having in my possession an old vacuum-gauge with a column three inches long, which had originally been wetted with sulphuric acid, but into which a considerable quantity of water had accidentally been introduced, I carefully allowed all the air to escape, and then applied a mercurial air-pump to the open end of the gauge, and exhausted as far as the pump would draw. The mercury did not descend. As I could apply no further tension, I shook the gauge up and down; but still the mercury remained unmoved. I then tapped the gauge smartly on the side; the mercury then fell three inches, until it was level. Having succeeded so far, I extracted the mercury and sulphuric acid from the 35-inch gauge and introduced some water without washing the tube, and, having boiled the water in the tube, again introduced the mercury.

Having extracted all the air, I found no difficulty in making the gauge to stand up to the 35 inches without any immediate tendency to fall. On applying the air-pump to the open end the mercury several times remained up until the exhaustion had proceeded so far that when it fell it fell from 22 to 28 inches, and when the rupture took place it was accompanied by a loud click. I could not on that occasion get the mercury to withstand complete exhaustion; but after leaving the gauge with the mercury suspended for 24 hours at 35 inches, I was able to exhaust the open end of the tube as far as the pump would draw, without bringing the mercury down; so that I had a column of 35 inches of mercury suspended by the cohesion of the liquids.

There was no reason to suppose that this was the limit or anywhere near the limit. It was clearly possible to suspend a longer column; but as the length of the column increased so would the difficulty of getting rid of the disturbing causes, and I determined to rest satisfied with the 35 inches; but in order to see if this could be maintained, I obtained a gauge 60 inches long, which would leave 30 inches above the pressure of the atmosphere.

The difficulty of getting rid of the air in this tube sufficiently to allow of the mercury standing 60 inches was very considerable. Before filling the tube it was rinsed out with concentrated sulphuric acid, then twice washed with distilled water, and then water put in and boiled in the tube. Then sufficient mercury was introduced to fill the long leg and the bend, so that the column, when complete, was 59 inches long, the barometer being at 29·5.

After the tube had been tilted several times so as to allow the air to pass out, the mercury would be suspended as the tube was slowly reerected, until it had attained an elevation of 40, 50, or sometimes the full height of 60 inches

(as shown in fig. 3), but only for a few seconds. When the mercury fell, if the column broke anywhere near the top of the tube, it gave way with a loud click. But this was by no means always the case. The mercury would sometimes separate nearly 30 inches down the tube; and

Fig. 3.

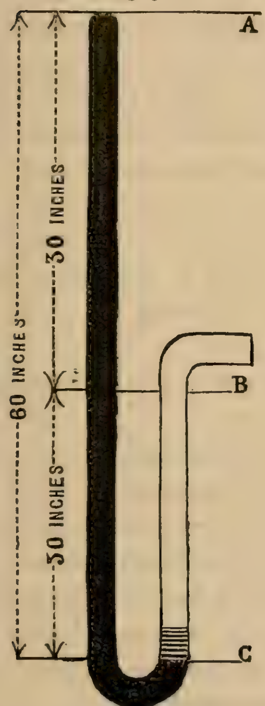


Fig. 4.

then the appearance of the upper portion falling was very singular: the upper portion of the column remained intact; and a stream of mercury fell from its under surface, as shown in fig. 4, breaking up into globules as it came into contact with the lower portion, with a loud rattling noise. I was unable to get the column in the tube thus filled

to maintain itself for more than twenty or thirty seconds, which failure was clearly due to the presence of air; for after the mercury had fallen a small quantity of air was always found to collect above it. Sometimes, when on inclining the tube the liquid again reached the top, the bubble which remained was so small as to be scarcely visible, although subject to no pressure other than the surface-tension; but its presence always became apparent instantly on erecting the tube. In no case was it possible, after the mercury had once fallen, to get it to remain up to any considerable height above that due to the pressure of the atmosphere until the bubble of air collected had been allowed to pass out.

The tube was then again emptied, washed, and filled with glycerine. This behaved much in the same manner as the water; but the difficulty of getting rid of the air was greater.

Similar results were obtained when very dilute ammonia-liquid was tried.

The tube was then again carefully washed, first with water, and then several times with concentrated sulphuric acid. The mercury was subjected to nitric acid, washed and dried, and then filtered into a bottle of sulphuric acid, from which it was poured into the tube, some acid passing in with the mercury. When first introduced into the tube a few small bubbles could be seen rising between the mercury and the tube and passing up through the sulphuric acid into the vacuum above; but after it had stood for five or six hours no bubbles were perceived, the surface of the mercury against the tube being perfectly clear; nevertheless, on erecting the tube, the mercury would not rise above the height of the barometer, and air was always found to have collected above the mercury. Water was then introduced so as to dilute the acid; then the mercury was

suspended as before, for a few seconds only. The tube was then placed in a position with the closed end lowest, so that the air and water might ascend towards the end and pass out; and after being in this position for some hours, when it was again erected the column remained intact.

It was thereupon again lowered and left to drain for forty-eight hours. On being again erected, the mercury was still suspended. The tube has since been carried in a more or less horizontal position some three miles to the Society's rooms in order that I might exhibit this phenomenon. If it has not been affected by the shaking, you will see a suspended column of mercury some fifty-nine inches high, or twenty-nine inches above the height due to the atmosphere*.

Conclusion.

The difficulty of obtaining a column of mercury thirty inches above the pressure of the atmosphere does not, I think, prove that the limit of the cohesive power of the liquid has been arrived at, or even the limit of the adhesive power of the water for glass and mercury, but simply shows that, although imperceptible, there are still bubbles of air in the liquid between the mercury and the glass which will not readily pass out.

It seems to me to be probable that, with sufficient care, or by using apparatus more suitable to the purpose, much greater heights might be attained. But however this may be, we have proof that mercury and water will, by their cohesion, resist a tension of at least one atmosphere, or that the common pump would, if the water were free from

* At the Meeting not only did the mercury remain suspended when the tube was erect, but on the pressure of the atmosphere being removed with an air-pump it still remained suspended, although the tension at the top of the tube was nearly equal to two atmospheres.

air, raise water by suction to a height of more than sixty feet. At first sight it cannot but appear remarkable that such a fact should for so long have escaped notice; but a little consideration removes the difficulty.

Water is almost always more or less saturated with air, which separates into bubbles as soon as the pressure is relieved; and in the common pump a single minute bubble would be sufficient to cause the column to break and prevent it being raised to a greater height than that due to the pressure of the atmosphere.

In the case of barometers it is the custom to fill the tubes full and boil the mercury, so as to get rid of the air; but the column falls to the usual height not by the rupture of the mercury, but by the separation of the mercury from the glass, for which it has but little adhesion. Whether the ordinary method of boiling the mercury really disengages all the air is, I think, an open question. In vacuum-gauges of small diameter it is not uncommonly found that the mercury sticks to the glass until the pressure has fallen considerably below what is represented by the height of the mercury, so that on the gauge being shaken the mercury falls with a sudden drop. Although it does not seem to have attracted any special notice, this phenomenon is clearly due to the same cause as that which I have found capable of maintaining thirty inches of mercury suspended in a comparatively large tube.

It would seem then that, although the facts which I now bring before the Society have little bearing on the practical limits to the height of the column of mercury in the barometer or the column of water in the common pump, they show that these limits are owing to the presence of air or some other minor disturbing cause, and are not, as seems to have been hitherto supposed, owing to the want of cohesion of the liquid. And it seems to me that the

cohesion now found to exist occupies an important as well interesting place in the properties of liquids.

APPENDIX (26th April).—*Previous Notices of the Cohesion of Liquids.*

Besides the hanging of mercury in small gauges, another phenomenon, which has long been known, shows a small degree of cohesion in water; that is, that water will rise up small tubes by capillary attraction as well in the receiver of an air-pump as in air at the ordinary pressure. This fact was shown before the Royal Society by Robert Hooke.

Prof. Maxwell, in his 'Treatise on the Theory of Heat,' p. 259, after commenting on the fact that water has been raised to a temperature of 356° F., without boiling, remarks:—"Hence the cohesion of water must be able to support 132 lbs. weight on the square inch," from which it would appear that he recognizes cohesion as a property of water, and considers that the possibility of raising the temperature above the boiling-point is evidence of such cohesion; but I am not aware that he has anywhere given his reasons for such a conclusion.

I am indebted to Dr. Bottomley for reference to a paper in the Ann. de Chim. et de Phys. (3) xvi. 167, by M. F. Donny, in which M. Donny gives an account of experiments in which he found that columns of sulphuric acid could be suspended *in vacuo* to a height of 1·3 mètre (about 50 inches), showing a tension of about 7 inches of mercury, care having been taken first to remove all the air from the acid. M. Donny further describes experiments made with water in exhausted tubes, in which he showed the effect of cohesion by shaking the tube. M. Donny does not, however, appear to have thought of the plan which I adopted of making mercury adhere to the tubes by wetting them with sulphuric acid or water. Not being

able to use mercury, the tensions which he obtained were comparatively small; and although he seems to have considered that greater tensions might be obtained, he mentions one or two atmospheres as probably possible. It would therefore appear that he had not conceived the possibility of the cohesion of liquids being comparable with that of solids.

M. Donny appears to have been influenced in adopting this limit to his idea of cohesion by a passage from Laplace, 'Mécanique Céleste,' Supplément au X^e livre, p. 3, which he quotes.

Laplace, who was the first to investigate systematically the phenomena of capillary attraction, proceeded on the hypothesis that the molecules of a liquid exercise attraction for each other at insensible distances only; and from this assumed attraction he deduces the surface-phenomena. The entire passage quoted by M. Donny is too long to introduce here; but the gist of it is comprised in the following extract:—

"*Son expression analitique est composée de deux termes: le premier, beaucoup plus grand que le second, exprime l'action de la masse terminée par une surface plane; et je pense que de ce terme dependent la suspension du mercure dans un tube du baromètre à une hauteur deux ou trois fois plus grande que celle qui est due à la pression de l'atmosphère, le pouvoir réfringent de corps diaphanes, la cohésion, et généralement les affinités chimiques.*"

Laplace here speaks of the suspension of mercury to 60 or 90 inches as if it were a well-known phenomenon; but I cannot find any reference to experiments, or, indeed, any further mention of the phenomenon in his memoir.

I did not refer to Laplace in the first instance, although I knew well that it is to him we are indebted for the theory of surface-tension almost in the form now accepted, because

I wished to avoid all reference to molecular hypothesis, and particularly the molecular attractions assumed by Laplace, lest it might in any way appear as if the conclusion that continuous liquids are as capable of resisting tension as solids (at which I arrived simply from considering the phenomena of surface-tension) were based on such assumptions. I was not aware, however, that Laplace had at all inferred or attempted to apply his theory to prove the ability of liquids to resist great tensions; nor do I find, on again reading his memoir, that he anywhere, with the exception of the almost casual reference quoted above, treats of such a property of liquids. His purpose appears to have been solely to explain the phenomena of capillarity. It appears obvious, moreover, that his line of reasoning must have forced upon his notice the conclusion that, according to his hypothesis, liquids ought to possess the property of very great cohesion; so that from the extremely slight notice which he has accorded to this property, one can only infer that he was not completely convinced of its existence.

II. *On the Combinations of Aurin with Mineral Acids.*

By R. S. Dale, B.A., and C. Schorlemmer, F.R.S.

Read December 10th, 1878.

In our last communication* we stated that by the action of acetyl chloride on aurin we obtained a colourless crys-

* Proc. Lit. and Phil. Soc. 1878, p. 141.

talline compound, which we intended to examine more closely. We have since found that this body is identical with a compound which Gräbe and Caro* obtained by the direct union of aurin and acetic anhydride and having the formula $C_{19}H_{14}O_3 + C_4H_6O_3$.

We also mentioned that the purification of this substance was found to be beset with several difficulties. The cause of this was found out after some trouble; but at the same time we were rewarded by the discovery of a series of remarkable bodies, consisting of combinations of aurin with mineral acids.

These salts, as we may call them, are beautiful bodies, crystallizing exceedingly well; and although some of them are decomposed by water, they are very stable in dry air. To their discovery we were led by the following observations.

On heating aurin with glacial acetic acid and acetyl chloride, the crystals lose at once their steel-blue lustre and assume a pale red colour. To obtain the compound thus formed in a pure state, acetyl chloride was added to a saturated solution of aurin in acetic acid. The liquid assumed at once a much lighter colour; and soon pale red needle-shaped crystals having a diamond lustre separated out. On recrystallizing these repeatedly from alcohol, we obtained oblong six-sided plates, which, as analysis showed were pure aurin.

On treating the original crystals with water, they became dull and brownish red, the solution containing acetic and hydrochloric acids. It therefore seemed not improbable that an additive product of aurin and acetyl chloride had been formed, containing, however, also acetic acid, as a superficial examination showed that the liquid contained, to one molecule of hydrochloric acid, much more than one

* Ber. deutsch. chem. Ges. xi. p. 1122.

molecule of acetic acid. We therefore tried to obtain an analogous benzoyl compound, and to determine in it, after decomposition with water, the relative quantities of hydrochloric and benzoic acids.

On adding benzoyl chloride to a hot solution of aurin in acetic acid, similar crystals as before were obtained, which, after being dried on filter-paper in dry air, were decomposed by water; but only hydrochloric and acetic acids went into solution, and on heating the product with water or alkalies but a mere trace of benzoic acid could be detected.

These facts, coupled with the observation that the bright-red needles formed (as we stated in our former paper) by crystallizing aurin from hot aqueous hydrochloric acid retain the latter obstinately, led us to the conclusion that this acid forms a definite compound with aurin.

Such a body could be formed under the above conditions, as our glacial acetic acid contained a little water. Moreover Mr. Charles Lowe had informed us that the splendid specimen of aurin which he exhibited at Paris was obtained in the following way. The crude but crystalline aurin which is obtained by heating pure phenol with sulphuric and oxalic acids was dissolved in alcohol, and some strong hydrochloric acid added, by which a crystalline precipitate was formed, crystallizing from hot acetic acid in beautiful red, glistening, flat needles. He was kind enough to give us a sample; and on examining it we found that water acted upon it in the same way as on our crystals.

In order to prepare a pure compound for analysis, a hot solution of aurin in acetic acid was saturated with hydrochloric acid gas. The colour of the liquid changed into a light yellowish red; and soon the compound separated out in glistening needles, which, even when perfectly dry, smell strongly of acetic acid. When exposed to the air, they soon assume a steel-blue lustre and gradually crumble into

a reddish brown crystalline powder. The same properties are shown by the crystals obtained from acetyl chloride and those obtained from Mr. Lowe. When heated to 110° in a current of dry air, they gradually lose all the acetic acid (which plays the part of water of crystallization) and assume a dull red colour.

On passing hydrochloric acid gas into an alcoholic solution of aurin, similar but smaller needles are formed, containing alcohol, which is given off at 100°; the dull red residue can, like the preceding one, be heated to 190° in a current of dry air without losing hydrochloric acid, which only begins to escape at 200°.

Analysis of these compounds showed that the dried substance consists of $C_{19}H_{14}O_3,HCl$, while the crystals obtained from an acetic-acid solution have the composition $C_{19}H_{14}O_3,HCl + 2C_2H_4O_2$, and those from alcohol $2C_{10}H_{19}O_3,HCl + 3C_2H_6O$.

When sulphuric acid is added to a hot alcoholic solution of aurin, small red needles are formed on cooling, which consist of $(C_{19}H_{14}O_3)_2SO_4H_2$ + alcohol. Under the same conditions an acetic-acid solution yields fine prismatic crystals, or flat very glistening needles, which are an acid sulphate, its formula being $C_{19}H_{14}O_3,SO_4H_2$ + acetic acid.

We have also prepared a nitrate (which is readily formed and crystallizes well), but have not analyzed it yet.

In our first communication to the Chemical Society we described a compound of aurin and sulphur dioxide, which is easily obtained in bright-red crystals by passing sulphur dioxide into a saturated alcoholic solution of aurin. Our former observation, that this body contains water but no alcohol, we found confirmed. On heating it decomposition easily takes place, pure aurin being left behind; but it appears to be quite stable when exposed to the air; and even on heating it with water, no sulphur dioxide is given off;

but a drop of sulphuric acid added to the mixture is sufficient to evolve the gas abundantly. Aurin sulphite has the composition $(C_{19}H_{14}O_3)_2SO_3H_2+4H_2O$.

As we have already showed, aurin forms very characteristic compounds with the acid sulphites of the alkali-metals, which, in accordance with the newly established formula of aurin, must now be written as follows :—

$$C_{19}H_{14}O_3,SO_3KH;$$
$$C_{19}H_{14}O_3,SO_3NaH;$$
$$C_{19}H_{14}O_3,SO_3(NH_4)H.$$

We have also found that rosolic acid, or the next higher homologue of aurin, forms compounds with mineral acids which crystallize well. Being, therefore, a base like aurin, we think its name ought to be altered; and as it has only been obtained from rosaniline, we propose for it the name *rosaurin*.

III. *On the Estimation of Small Excesses of Weight by the Balance from the Time of Vibration and the angular Deflection of the Beam.* By J. H. Poynting, B.A., B.Sc.

Read December 10th, 1878.

While working last year on an experiment to determine the mean density of the earth by the balance, I had to measure such an exceedingly small difference of weight that I could not at that time estimate it by means of a rider,

but was obliged to adopt the method described in this paper. Stated generally, it consists in treating the balance as a pendulum. Knowing the nature of the pendulum (that is its moment of inertia) and its time of vibration, we can calculate what force acting at the end of one arm of the beam will produce a given angular deflexion. It is, in fact, an application to the common balance of the method which has always been used with the torsion-balance when it has been necessary to calculate the forces measured in absolute measure. I cannot find any record of a previous application of the method; and as it might be of use in very delicate weighings or in verifying the small weights in a laboratory, I have thought it worth while to give a full account of it.

When small quantities of the second order are neglected and the oscillations are of the first order, it will easily be found that the equation of motion of the beam of the balance is

$$\left(\mathrm{MI}^2+\frac{2\mathrm{P}a^2}{g}\right)\ddot{\theta}+(2\mathrm{P}h+\mathrm{M}gk)\theta=ap, \quad . \quad . \quad (1)$$

where MI^2 = moment of inertia of beam about central knife-edge,

M = mass of beam,

a = half length of beam,

P = weight of either pan and the mass in it,

h = distance of line joining terminal knife-edges below the central knife-edge,

k = distance of centre of gravity of beam below central knife-edge.

p = small excess in one pan.

θ = angular deflection in circular measure produced by p,

g = gravity.

If $\ddot{\theta}=\theta$, we have the position of equilibrium given by

$$\theta=\frac{ap}{2Ph+Mgk}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

The semiperiodic time is

$$t=\pi\sqrt{\frac{MI^2+\frac{2Pa^2}{g}}{2Ph+Mgk}}. \quad . \quad . \quad . \quad . \quad (3)$$

From equations (2) and (3) we can eliminate $2Ph+Mgk$, obtaining

$$p=\pi^2\frac{MgI^2+2Pa^2}{ag}\ \frac{\theta}{t^2}. \quad . \quad . \quad . \quad . \quad (4)$$

From this expression it appears that, if we know the moment of inertia of the beam, its length, and the weight at each end, we can find the excess p from the time of vibration and deflection.

The results given in this paper were obtained with a 16-inch chemical balance by Oertling. The exact length of the half beam (a) measured by a dividing-engine is 20·2484 centimetres.

To find the Moment of Inertia MI^2 *of the Beam.*—The simplest way theoretically would appear to be this. Find the times of vibration t_1, t_2, and the deflections θ_1, θ_2, due to the same excess p with two different loads P_1, P_2 in each pan. Equating the values of p given for the two by equation (4) we have

$$\frac{MgI^2+2P_1a^2}{MgI^2+2P_2a^2}=\frac{\theta_2 t_1^2}{\theta_1 t_2^2},$$

an equation which will give MgI^2 in terms of known quantities; but on trial it was found that a very small

proportional error in the observed time made a large error in the value of MgI^2; and the following method, that usually adopted in magnetic observations, was employed in preference. A stirrup was suspended by a platinum wire, and its time of vibration (t_1) against the force of torsion (μ) of the wire was observed. The moment of inertia of the stirrup being S, we have

$$t_1^2 = \frac{\pi^2 S}{\mu}.$$

The time of vibration (t_2) was then observed when a cylindrical brass bar of known moment of inertia (B) was inserted in the stirrup. We now have

$$t_2^2 = \frac{\pi^2}{\mu}(S+B).$$

The bar was then removed and the balance-beam inserted in its place; and the time of vibration (t_3) gives

$$t_3^2 = \frac{\pi^2}{\mu}(S+MI^2).$$

From these three equations, eliminating S and μ, we obtain

$$MI^2 = \frac{B(t_3^2 - t_1^2)}{t_2^2 - t_1^2}.$$

Now Bg was calculated from the weight and dimensions of the bar to be 6332·83 (in centimetres and grammes). The observed times were $t_1 = 3{\cdot}6792^s$, $t_2 = 4{\cdot}495^s$, $t_3 = 7{\cdot}1483^s$. From these values we find

$$MgI^2 = 35651{\cdot}6*$$

* To this a small correction should be added if the adjusting-bob is not in its lowest position. This amounts to 7·6 for each turn of the screw, and may therefore in general be neglected.

To measure θ.—The angle of deflection was measured by the number of divisions of the scale which the pointer moved over. As the length of the pointer is 32·1006 centimetres, while 20 divisions of the scale measure 2·5658 centimetres, a tenth of a division, in terms of which the deflexion was measured, corresponds to an angle of 0·0003996°. The oscillations were observed from a distance of six or eight feet by a telescope. The resting-point (*i. e.* the point where the balance would be in equilibrium) was found in the usual way by observing three successive extremities of two swings and taking the mean of the second and the mean of the first and third. Five determinations of the resting-point were usually made with the excess to be measured alternately added and removed. From these five, three values of the deflection (n) due to the excess were calculated in a manner which will be seen from the example below.

The Time of Vibration.—This was found from several determinations of the time of ten oscillations. The method will be seen from the example. No correction was needed for the resistance of the air as long as the vibrations did not exceed two divisions of the scale. When, however, they were much more than that, the time of vibration was found to increase with the arc. As the time of vibration frequently changes slightly, probably through variations of temperature, it was usually observed before and after the determination of the deflection (n) and the mean of the two taken as the true time.

The following example of the determination of the value of a centigramme rider by placing it halfway along the beam will sufficiently explain the details of the method.

Time of Vibration at Commencement.

No. of vibration.	Observed time of passage of pointer through resting-point.	No. of vibration.	Observed time of passage of pointer through resting-point.	Time of 10 vibrations.
	Pointer apparently moving from left to right.			
	h. m. s.		h. m. s.	s.
0	11 15 36	10	11 17 43	127
2	11 16 1	12	11 18 8	127
4	11 16 26·5	14	11 18 33·5	127
6	11 16 52	16	11 18 59	127
	Mean value of 10 vibrations			127
	Pointer apparently moving from right to left.			
1	11 15 49	11	11 17 56	127
3	11 16 14	13	11 18 21	127
5	11 16 39·5	15	11 18 46	126·5
7	11 17 5	17	11 19 11·5	126·5
	Mean value of 10 vibrations........................			126·75

Mean of means $=126·875$; $t_1=12·6875^s$.

Determination of Deflexion n.

Excess weight	Extremities of oscillation.	Resting-point.	Mean of preceding and succeeding resting-points.	Deflection due to excess.
Added	109 96 109	102·5		
Removed ...	93 40 92	66·25	102·25	36
Added	152 53 150	102	66·75	35·25
Removed ...	80 55 79	67·25	102·5	35·25
Added	147 60 145	103		

Mean value of $n=35·83$.

Time of Vibration at end.

No. of vibration.	Observed time of passage of pointer through resting-point.	No. of vibration.	Observed time of passage of pointer through resting-point.	Time of 10 vibrations.
	Pointer apparently moving from left to right.			
	h. m. s.		h. m. s.	s.
0	11 26 19	10	11 28 27	128
2	11 26 44·5	12	11 28 53	128·5
4	11 27 10	14	11 29 18	128
6	11 27 35·5	16	11 29 44	128·5
	Mean value of 10 vibrations........................			128·25
	Pointer apparently moving from right to left.			
1	11 26 32·5	11	11 28 39	126·5
3	11 26 58	13	11 29 5	127
5	11 27 23·5	15	11 29 30·5	127
7	11 27 49	17	11 29 56·5	127·5
	Mean value of 10 vibrations........................			127

Mean of means $=127{\cdot}625$; $t_2=12^{s}{\cdot}7625$.

Remembering that one tenth of a division of the scale is an angle of ·0003996° in circular measure, formula (4), expressed in milligrammes, becomes

$$p=\frac{n}{t^2}0{\cdot}3996\frac{\mu^2}{ag}(Mg\mathrm{I}^2+2\mathrm{P}a^2).$$

In our present example*

$$n=35{\cdot}83,$$

$$t=\frac{t_1+t_2}{2}=12^{s}{\cdot}725,$$

* For this, as for several other cases, I removed the pans and hung the weights directly by fine wires from the suspending-pieces. By this means the resistance of the air was very much diminished.

$$MgI^2 = 35651,$$

$$2Pa^2 = 94704,$$

$$p = 5{\cdot}724 \text{ milligrammes.}$$

The length of time occupied in this determination was not quite a quarter of an hour.

The following table contains a series of results which I have obtained of the weight of two centigramme riders, the first of which was accidentally destroyed after the conclusion of the fourth determination. As the rider was always placed at division 5 on the beam, the values given in the table are double those actually obtained.

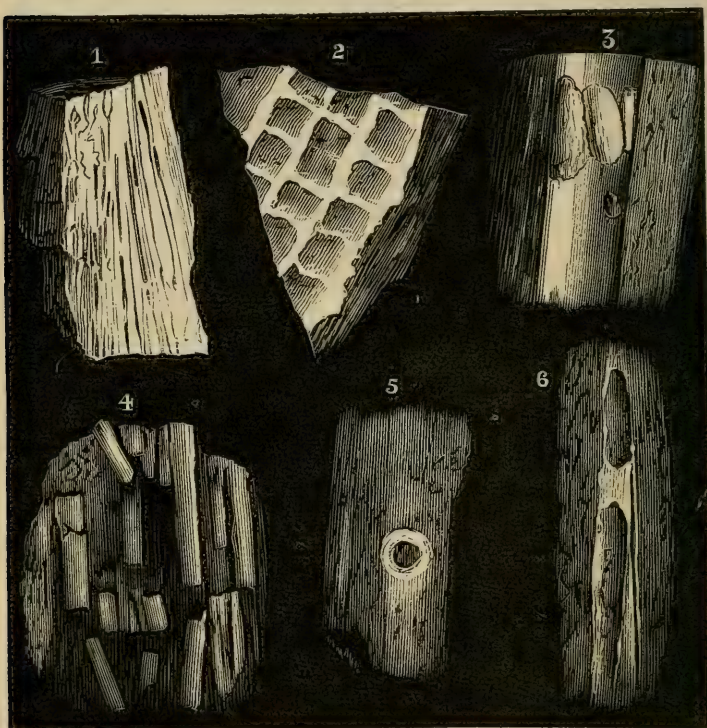
No. of experiment.	$MgI^2 + 2Pa^2$	t in seconds.	n.	Weight of rider in milligrammes.	Mean value.
1	145364	8·921	13·458	9·78	9·96 milligrammes.
2	309356	17·65	25·49	10·05	
3	519769	20·435	19·12	9·55	
4	130355	13·10	34·71	10·47	
5	130355	12·87	36·6	11·44	11·35 milligrammes.
6	130355	12·72	35·5	11·35	
7	130355	12·725	35·83	11·45	
8	130355	12·81	35·5	11·20	
9	130355	12·903	36·37	11·31	
10	454405	19·406	22·08	10·58	

IV. *On Siliceous Fossilization.*—Part II. By J. B. HANNAY, F.R.S.E., F.C.S., Assistant Lecturer on Chemistry in the Owens College.

Read March 18th, 1879.

IN a former paper it was shown, by chemical and optical means, that the fossil siliceous rods *Hyalonema Smithii* were identical in constitution with those from modern sponges, and that the curious nodulized appearance of some of the rods was due, not to the original form of the rods, but to certain physical and chemical changes which have passed over them since they were deposited where they were found. It was also shown that of the three forms of silica, transparent, gelatinous, and opaque, the first and second were easily acted upon and retained the original structure of the organic silica, whereas the last was in the truly mineral form and had lost every trace of organic structure, and was not easily acted upon by chemical means. Mr. John Young, F.G.S., having kindly supplied me with several specimens of the fossils from the limestone quarry at Kilwinning, which are very different from those I previously examined, and which throw more light on the changes which siliceous fossils may undergo, I beg to give an account of them to the Society. One piece of limestone simply contained (instead of rods) a number of cylindrical holes where the rods had lain. In fig. 1, which is a woodcut from a photograph, are seen these cylindrical holes, which plainly show that the rods have been dissolved away. The solvent must have been a strong calcareous or other alkaline solution, as the calcareous fossils are not in the

least disfigured. In fig. 2 we see a beautifully preserved sample of *Fenestella*; and we know that a very slight solvent action would have destroyed the structure of these delicate organisms. From other internal evidence, such as little shells and diatomaceæ, it is clear that the calcareous



portion of this limestone has not been at any time dissolved to any extent; and yet such an obdurate substance as silica has been completely removed. Then, as to the cause of its removal. The solution was no doubt highly calcareous; but we know that highly calcareous water may run over quartz crystals for a very long period without having the slightest effect upon the faces. I think that fig. 3 will

explain how the rods came to be so easily dissolved. This is a photograph of a hollow where a rod has lain which still contains some rounded nodules of silica. It will be seen from my former paper that these nodules are anhydrous inorganic silica, crystallizing out of the hydrated silica after the rod has undergone a little dehydration since it was alive. Now here we see the whole rod dissolved except such portions as were entirely mineralized, if I may use such a term; so that the reason the silica rods were so easily dissolved by the calcareous solution was because the silica of which they were composed was in a hydrated easily soluble form. Thus the existence of those nodules which had before puzzled naturalists now gives us the clue to the state of the rods at the time of solution. Fig. 4 will still further elucidate this subject. Here it will be seen we have a large number of rods partially dissolved. I have examined, by the means given in my former paper, above twenty samples of these partially dissolved rods, and I have not found one sample containing water; so that again we see the whole of the thoroughly mineralized silica is left behind, and those portions which were very probably hydrated dissolved. I say very probably; for we see that the solvent action has gone on in a very irregular manner, and in a manner which could not be accounted for on any circulation-hypothesis, but just in such a manner as would be caused by the irregular manner in which the rods get mineralized.

It might be expected that, since the rods were so neatly and perfectly dissolved out, the spaces might get filled up with carbonate of lime and reproduce the silica rod as a calcareous fossil; but, although I have examined some hundreds of these fossils, I have not found one case of this nature. The only case I have found even approaching to this is, that when the centre of the rod is dissolved it is

sometimes, as in fig. 5, filled in with carbonate of lime. I have noticed that when carbonate of lime is deposited in the cavity where the rod lay it is highly crystalline, and could never be mistaken for any thing organic. Fig. 6 is from a photograph showing the carbonate of lime in the silica-centre.

As the above remarks border on a subject which has been discussed very extensively, I may be allowed to point out that they settle one half of the discussion, namely that silica may be dissolved in presence of calcareous fossils; but the other half, namely whether or not the spaces so left may be filled up with carbonate of lime so as to look like fossils, is still an open question.

V. *On the Mean Temperatures of the Winters of the last Twenty-nine Years.* By the Rev. THOMAS MACKERETH, F.R.A.S. &c.

Read before the Physical and Mathematical Section, February 25th, 1879.

IT may be considered somewhat premature to institute a comparison of the mean temperatures of the last twenty-nine years when the colder of the extremes of the winter months has not yet been entered upon, viz. the month of March. In a meteorological sense winter may be considered as including five months of the year, viz. November, December, January, February, and March. But January,

so far as low temperature is concerned, is the pivotal month of the winter, and March has a mean temperature slightly below that of November. The difference is so small, only about 0°·7 Fahr., that in comparing the mean temperature of winters, the mean temperature of these two months may be practically neglected. The mean temperatures deduced from my own observations extend over only 17 or 18 years; but the late G. V. Vernon, Esq., F.R.A.S., was kind enough to furnish me with the weekly temperatures he had deduced from the year 1850 onwards till 1861, when I began to make my own observations and deductions. That the mean temperatures here presented may have a common basis, I have calculated them upon the weekly mean temperatures of the last 29 years, which, of course, include those of the late Mr. Vernon.

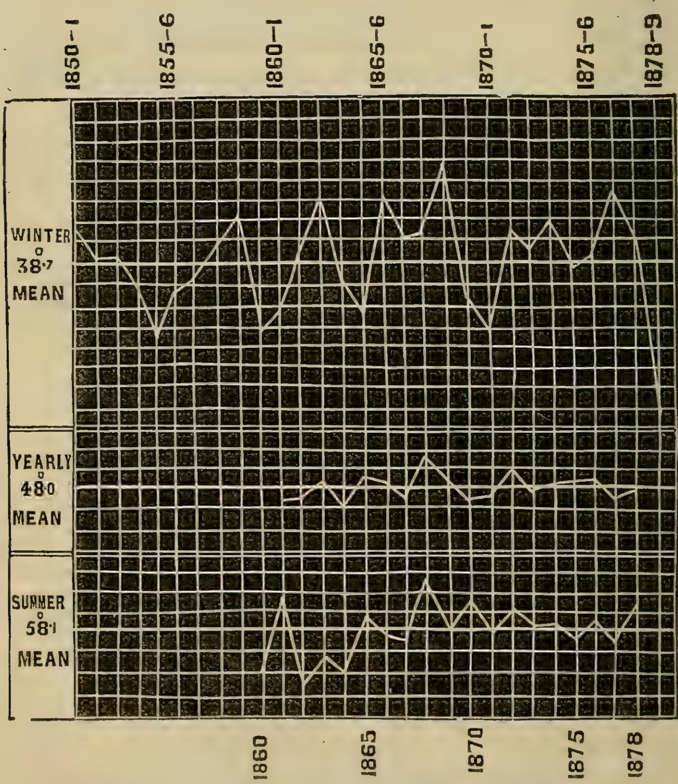
The results are as follows for the winters extending from the first week of December in one year to the last completed week in February of the year following:—

Winter of	Mean temperature.	Winter of	Mean temperature.
	°		°
1850–1	40·3	1865–6	41·9
1851–2	39·8	1866–7	39·9
1852–3	39·9	1867–8	40·0
1853–4	37·3	1868–9	43·6
1854–5	35·1	1869–70	37·1
1855–6	37·3	1870–1	35·6
1856–7	37·9	1871–2	40·3
1857–8	39·4	1872–3	39·2
1858–9	41·0	1873–4	40·6
1859–60	35·4	1874–5	38·6
1860–1	36·4	1875–6	38·8
1861–2	39·3	1876–7	42·3
1862–3	41·9	1877–8	39·6
1863–4	37·9	1878–9	31·4
1864–5	36·6		

From the above table of winter mean temperatures it

will be seen that the coldest winters of the last 29 years were in 1854–55, 1859–60, 1870–71, and in 1878–79, and that the present winter is colder than the coldest of the preceding, viz. 1854–55, by 3°·7 or 12 per cent. The mean temperature of the winter months for 29 years is 38°·7. And whilst the winter of 1854–55 was 3°·6 or a little over 9 per cent. below the average winter-temperature, the present winter is 7°·3 or 19 per cent. below the average.

The accompanying diagram sets forth the ratio of all the winters from 1850–51 :—



Here it will be seen that five years elapsed between the cold winters of 1854–55 and 1859–60, but that six years elapsed between the cold winters of 1864–65 and 1870–71, and that eight years elapsed between the present cold winter and the previous one of 1870–71. It is now known that the sun-spot period is irregular and not so nearly an interval of ten, eleven, or twelve years, as was imagined. The minimum of the sun-spot period happened about two years ago; but it is still at a minimum, and very seldom have spots been seen on his disk during the past year. Whether these cold winters are traceable to this solar inactivity or not, the present coincidence is very striking.

In the same diagram I have presented the ratios of the mean summer temperatures of the last eighteen years. These ranges are included in the weekly mean temperatures of June, July, and August, and will be seen to be far less than the ranges of the winter mean temperatures. This, perhaps, may be accounted for by the relative difference of the amount of atmospheric vapour existing in the air in the two opposite seasons of the year. In the winter season the ratio of atmospheric vapour reaches 87 per cent., whilst in the summer season it reaches only about 75 per cent. But whether this is so or not, there is the fact. Here it will be seen that between the hot summer of 1861 and the still hotter one of 1868 there is an interval of seven years; since then the coming summer will make an interval of eleven years. In the interval of seven years there was only one summer with the temperature slightly above the average, 1865; all the other summers of this interval were mostly far below the average; but in the eleven years' interval all the summers, with only two exceptions, have either been above, or equal to the average summer temperature. The ratios of the summer and winter temperatures have not the slightest relationship

to each other. Hence it is impossible to form any idea from the temperature of a winter what kind of summer temperature may follow.

I have shown on the same diagram the mean temperature of each year from 1861–1878. The mean of all is 48°. The ratios of their differences are extremely small, so small, indeed, as to be of no practical value; for between the coldest year and the warmest of the last seventeen years there is a difference of only 2°·9, and the mean difference of temperature of all these years amounts to only 0°·3. These differences do not, from the data I have, seem to observe any definite rule.

VI. *Colorimetrical Experiments.*—Part II.

By James Bottomley, B.A., D.Sc., F.C.S.

Read before the Physical and Mathematical Section, April 22nd, 1879.

In a short note which I read before this Society (vol. xv. p. 63) I proposed to measure quantities of colouring-matter in solution, using the formula $qt = q't'$ in the calculation, q and q' denoting quantities of colouring-matter, t and t' lengths of columns of coloured fluid, the colouring-matters being dissolved in equal volumes of water. Last session I gave the results of some experiments which I had obtained two years previously. Lately I have made some further experiments, which I give in this paper, along

with some additional remarks on colorimetry. In my last paper I took ·0001 gram as the unit of measurement. For comparison I have retained it in this, although the quantities used can no longer be considered as traces. The colouring-matter used was ammonio-sulphate of copper. A solution was made by dissolving 10 grams of crystallized sulphate of copper in a mixture of 200 cubic centimetres of water and 50 cub. c. of ammonia. Mixtures of various degrees of intensity were made by taking portions of this solution and mixing with water so as to make up 500 cub. c. As in my last paper, A denotes the amount of the colouring-salt present, B the length of the column of fluid, and C the amount of the colouring-salt thence derived by calculation.

Standard solution 4000 in 500 cub. c. of water, depth of disk 8·3 :—

A	B	C
6000	4·3	7721,

a result considerably remote from the correct value. Also when the disk was placed inside the solution at the depth given by theory, it seemed too dark. Next, a comparison was made by looking through the cylinders at external white surfaces; standard solution 4000 in 500 cub. c. of water, length of column 8·4 :—

A	B	C
6000	5·1	6588.

Thus the result is considerably different from the real value. Also both experiments concur in giving too high a value. The theoretical depths, moreover, were tried with external surfaces, and seemed slightly too great. My next experiments were made with solutions containing 2400 and 1600 of sulphate of copper. The disk being inside, the

results were as follows; standard solution 1600 in 500 cub. c., depth of disk 8·3 :—

A	B	C
2400	5·6	2371.

The number under B was the result of seven trials. The value under C is a fair approximation to the real value. I also tried external surfaces with these solutions; standard solution 1600 in 500 cub. c., length of column 8·4 :—

A	B	C
2400	6·2	2161

Somewhat further from the correct value than we might expect. I also tried the theoretical length of column; with external disks it appeared a little too light. The results obtained with the last standard solution are inconsistent with the results obtained with the first. In the latter case the results are too low, while previously they were too high. Errors of observation arising from imperfect perception of colour, from imperfection of instruments and unfavourable conditions of light (for many of the experiments were made during the winter months, sometimes on gloomy unfavourable days) would, no doubt, contribute to this result; yet, allowing for all these, there seemed to be some other cause. In my last paper I mentioned that an ammoniacal solution of copper, when largely diluted, became turbid, and that to carry out the experiment an additional quantity of ammonia was necessary. This small quantity was added at hazard, as I did not think it would have any influence on the result. It seemed to me afterwards to be a point worth examining in connexion with the above experiments. Two solutions were made, each containing 5 cub. c. of the previously mentioned copper-solution with 245 cub. c. of water. The solutions were

placed in similar cylinders : to one of the cylinders more ammonia was added; it appeared perceptibly darker than the other. Hence it appears that the excess of ammonia has some influence on the result. I presume that ammonio-sulphate of copper has a tendency to be decomposed by water, and that some change is effected even before it becomes obviously marked by the formation of a turbidity; moreover it seems likely that the excess of ammonia has the power to counteract this property of water and to restore the original compound. Two solutions were made, the bulk of each being 545 cub. c.,—one containing 4000 of copper sulphate along with an additional 20 cub. c. of ammonia, the other containing 6000 of copper sulphate with 30 cub. c. of additional ammonia. The comparisons were made in new cylinders graduated to millimetres. An experiment with white surfaces external gave the following results; standard solution 4000 in 545 cub. c. of water, length of column 23 centimetres :—

A	B	C
6000	15·5	5955

Thus the result is very near the real quantity. I also took shorter lengths of the standard solution, namely 18, 13, and 8 centims.; the corresponding lengths of the other solution were 12·4, 8·5, and 5·3 centims. Reduced to 23 centims. of the standard, the lengths would be 15·8, 15, and 15·2, numbers not far removed from 15·5, which was got by observation. I repeated the experiment with fresh solutions, the bulk of the liquid being 500 cub. c.; standard solution 4000 in 500 cub. c., length of column 21·2 :—

A	B	C
6000	14·1	6000

The number under B is the theoretical quantity, and was

the mean result of four trials. Also with shorter columns of the standard liquid, namely 15·2, 10·2, and 5·2 centims., the corresponding lengths of the stronger liquid gave similar tints. At the same time lengths differing a little from the theoretical would also satisfy. With these solutions I also tried an experiment with disks inside; standard solution same as last:—

A	B	C
6000	12·3	6894.

The number under B was the mean of 15 trials; thus the result with disks inside was not so good as with disks outside. Also 14·1 centims., the theoretical depth, when tried, seemed slightly too dark with disks inside. I repeated the experiment with a solution containing 2400, using one containing 1600 as a standard. In preparing these solutions, to the stronger I added 12 additional cub. c. of ammonia, and to the weaker 8. With external white surfaces the results were as follows; standard solution 1600 in 500 cub. c. of water, length of column 21·2 centims.:—

A	B	C
2400	13·8	2458

The number under B was the result of fifteen trials. Thus we get a good approximation to the real quantity. With disks inside the results were as follows; standard solution same as last:—

A	B	C
2400	12	2824

The number under B was the mean of twelve trials. The value got with disks inside is not so good as with disks outside. Also the theoretical depth 14·1 centims., when tried, seemed to be slightly too great. From the foregoing

experiments it would appear that with considerable additions of ammonia the results with disks outside were much improved. Why the results were not equally good with disks inside may, I think, be accounted for, and will be considered further on. I also compared the solution containing 4000 with the one containing 1600; standard solution 1600 in 500 cub. c., length of column 21·2 :—

A	B	C
4000	8	4240

The number under B was the mean of eight trials. The theoretical length was 8·5. With the standard solution on the left hand this length seemed to give a similar tint; but with standard solution on right it seemed a little darker. And now I remarked for the first time, or, if I had previously remarked it, had not thought it worthy of notice, that even an apparently so trivial circumstance as altering the positions of the cylinders from right to left had a perceptible influence in the determination of colour. I afterwards made some experiments to test this. Next I compared solutions containing 1600 and 600; standard solution 1600 in 500 cub. c., length of column 21·2 :—

A	B	C
6000	5·1	6682

The number under B was the mean of eight trials. The result is not so satisfactory as the others. I also tried these solutions with white surfaces inside; standard solution same as last :—

A	B	C
6000	3·9	8697

—a widely remote result, and much further from the real value than with disks outside; also a disk placed in the solution at the depth assigned by calculation seemed too dark.

It seemed possible to me that a more satisfactory result than this experiment had yielded might be obtained. The excesses of ammonia used in the experiments were nearly proportional to the quantities of sulphate of copper in solution; but if we regard water as an agent whose tendency is to diminish the intensity of the colour, and ammonia as an agent whose tendency is to restore the colour, it would seem reasonable that the ammonia should be proportional to the water. The difference of the excesses of ammonia in the last two solutions was large, being 22 cub. c. I prepared fresh solutions, one containing 4 cub. c. of the copper-solution with 30 cub. c. of additional ammonia and sufficient water to make 500 cub. c. The other solution contained 15 cub. c. of the copper-solution with 30 cub. c. of additional ammonia, and sufficient water to make 500 cub. c. The quantities of the copper-solution taken should correspond to 1600 and 6000 of copper-sulphate. To guard against imperfect measurements from the burette, I also weighed the solutions: the 4 cubic centimetres weighed 3·9854 grams; and the 15 cubic centimetres weighed 14·99 grams. The ratio of the volumes is 3·75, and the ratio of the weights is 3·761; so that the error of measurement would be but small.

With disks outside, the results of experiments were as follows; standard solution 1600 in 500 cub. c. of water, length of column 21·2:—

A	B	C
6000	6	5653

The number under B was the result of eight trials; also the standard solution was on the left hand. With the standard solution on the right the results were:—

A	B	C
6000	5·4	6283

The number under B was the mean result of eight trials. In one case the value got by experiment is too high, and in the other too low. The theoretical length is 5·65; and 5·4 is not very far from it. When I actually tried a column of the theoretical length, it seemed to give the required tint when the standard solution was on the left; when the standard solution was on the right it seemed a little darker. The mean of the two values previously obtained is 5968, which is near to the real value.

From the above experiments it seems that, when excesses of ammonia are added, very fair approximations may be obtained by colorimetry to the quantity of copper-sulphate in solution, the white surfaces being external; also that with white surfaces internal the results are more remote. In my last paper I stated that, when there was much difference between the standard solution and the one to be compared with it, the discrepancies when internal disks were used were considerable. Then I was using very small quantities of colouring-matter. From these experiments, where the colours were intense and the quantities of colouring-matter used considerable, a similar conclusion follows. The reason for this is not far to seek, and it also suggests a correction that must be applied to the formula when the white surfaces are inside. A white disk inside a column of coloured liquid looks darker in colour than a white disk outside placed a few inches below a column of the same length. This may be tried by looking through different columns, or through the same column, and having inside a white disk of smaller diameter than the cylinder; the inside disk will then appear surrounded by a rim of lighter colour. When the disk is internal, it is evident that the light which illuminates it has previously passed through the solution, so that we are looking not at a white disk, but at a coloured disk, through a coloured solution.

Some allowance must be made in the calculation for this coloration of the disk. The formula $qt=q't'$ is applicable to the case in which the surfaces are outside; to adapt this formula to the case when the disks are inside, suppose x to be the length of the column of fluid which would cause the difference in colour between an external and an internal surface, then the formula would be $q(t+x)=q'(t'+x)$.

To find the value of x experimentally, I took a solution containing 2400, and sunk a small disk in it until the inside and outside colour seemed the same: for the outside the length of column was 22·5, for the inside 17·7, this being the mean of eight trials. The difference is 4·8. With a solution containing 1600, a white surface outside, with length of column 21·2, seemed to give the same colour as white surface inside with length of column 16·2; the difference is 5. I also tried to get the value by the following combination:—A solution was taken containing 2400 with disk inside, and compared with a solution containing 1600 with a disk outside. Length of column in latter case was 21·2; in the former case a column 17·5 seemed to give a similar colour. From the formula $q(t+x)=q't'$ the resulting value of x would be 5·2. I also tried the following combination:—Solution containing 2400 and disk outside was compared with solution containing 1600 with disk inside. The mean of eight trials gave length of column 17 in the latter case, equivalent to 21·2 in the former; from the formula $1600\ (17+x)=2400\times 21{\cdot}2$, the resulting value of x is 4·3. Finally, we get for the approximate value of x, taking the mean of the four determinations, $x=4{\cdot}8$. The experimental determination is not easy; but the value obtained gives better results when we use it in the formula. For instance, on a former occasion, with disks inside, when a solution containing 1600 and length of column 21·2 was used as a standard, and there was compared with it a so-

lution containing 2400, the length of column was 12; from the uncorrected formula the result is 2824; from the formula

$$q'(12+4{\cdot}8)=1600(21{\cdot}2+4{\cdot}8)$$

the resulting value of q is 2476, which is not far from the proper value.

When the fluids compared differ much in strength, the value of the correction will probably vary. It will be better not to have the difference large, whether the disks be external or internal; for when the differences are large, any errors in the determination of the lengths of the columns have a greater effect on the calculated result.

I also tried to make a rough estimate of the length of the cylinder which might be covered without any perceptible darkening of the disk. I found that a black cloth cover investing the cylinder, might be drawn down until it was about 3·5 or 3·2 centims. from the disk; this would vary with the dimensions of the window and the relations of the cylinder to it. Also the length given is less than the value of x previously deduced: but it ought to be so; for the light from a vertical window to illuminate a horizontal disk must pass obliquely through the solution. It would also follow that parts of the disk more remote from the window are darker than parts nearer; hence, if the cylinders are of moderate radius, either the disks should be small and should be kept with their centres moving along the axes of the cylinders, or, in the case of larger disks, the determination of colour should be made by a comparison of similar parts of the disks.

In the estimation of colour, it is also not a matter of indifference, when we are given any tint as a limit, how we approach that limit. Suppose we have two cylinders, α and β, full of coloured liquid, that in α being the darker.

Now pour out from α until the colour seems the same as in β; before reaching the theoretical division sight will fail to discern any difference of colour. Now, if we proceed cautiously, as we approach the limit there will be a natural hesitation and tendency to stop; and it seems likely that in most cases, in obedience to that feeling, we shall stop with a column a little too long. Now suppose we start with the cylinder α empty and pour fluid into it; as we again approach the limit cautiously we shall again have a tendency to stop, and, inasmuch as before reaching the limit we pass through tints which we cannot distinguish from it, we are likely to take a column of fluid too short. In my own case I have noticed this on several occasions. For example, in trying to estimate a particular colour, the mean of seven trials made by pouring out from the cylinder gave 14·1 as the length of the column, the mean of seven trials made by pouring into the cylinder gave 13·7 as the length; and with disks inside, the mean of six trials made by moving the disk from below upwards gave 12·5 as the length of column, while moving from above downwards the mean of six experiments gave 11·7. In trying to estimate a colour, it seems to me that it would be well to approach the limit by both ways, and then take the mean of the results.

In a previous part of this paper I stated that altering the position of the cylinders made a little difference in my perception of colour. I made some experiments to try this. I took a solution containing 2400, and poured from the cylinder on the right hand into the cylinder on the left hand; the columns ought to be equal. The mean of nine trials gave length of column on right hand 11·2, length of column on left hand 10·61. In these experiments the judgment was made using both eyes. I next tried using one eye only. With the right eye the results were, length of column on right hand 10·86, length of column on left

hand 11·09; these numbers were the mean of nine trials. With the left eye alone the results were, right-hand cylinder 10·78, left-hand 11·01, being the mean of nine trials. Thus, using one eye only, the results are nearly the same in both cases; they also tend to made the right hand a little less, thus reversing the case of two eyes. These experiments were made in a room with a small window facing the south. I afterwards repeated the experiments, using two eyes, in another room having a window of larger dimensions and facing the north. A solution was used containing 1600; the mean of nine trials gave, right-hand cylinder 11·16, left-hand 10·55, nearly the same results as I got before. Why I should have this tendency to make one column a little longer than the other I do not know; possibly it may be some peculiarity of vision confined to myself. In the course of my experiments I have also noticed the following curious phenomenon, and this repeatedly, when working with solutions coloured with bichromate of potash and with ammonio-sulphate of copper:—Look steadily with one eye (say, the right) through the solution at a white surface, after the lapse of about a minute suddenly turn the head so as bring the left eye close over the cylinder; then the colour will seem more intense than it did with the right. Having looked with the left eye for about a minute, bring again the right eye suddenly close over the cylinder, and the colour will seem more intense than it did with the left, and so on alternately. It would seem as if the first impressions of colour on the eye were the stronger, and as if there were a gradual and imperceptible decrease in intensity. Perhaps alterations in the aperture of the pupil may contribute to this.

Another matter for consideration in colorimetry is the nature of the incident light. On some occasions we have the light from a blue sky; on other occasions the sky is

invested with clouds of various depths of grey, or sometimes tinged by the sun with a variety of tints, from yellow to red; while the light of the sun itself is frequently yellow or orange. All these variations of light are likely to have some influence on our judgment of colour, especially when the tints to be compared are light. Of the disturbing influence of colour in the incident light any one may convince himself by comparing yellows on a morning when the sky is enveloped in a yellow fog. In some experiments which I made with bichromate of potash during such fogs I found it much more difficult to decide at what depth equality of colour was effected; the disk in the stronger solution could be moved through a very considerable range without any change of colour being perceived. A similar result happened when I hung up yellow screens and tried to make determinations of colour behind them; also when looking at light-yellow external surfaces, differences in the lengths of the columns failed to give any differences in tint, although when looking at white external surfaces they did so. But in quantitative determinations of matter by colorimetry, the excellence of the results require sensible variations in colour when we alter slightly the length of the column; hence, when the incident light is tinged with the colour we wish to determine, the advantage of the method is diminished. Such a consequence may also be deduced from the formula which I obtained in my last paper. For suppose white light to consist of yellow, blue, and red (as far as the reasoning is concerned, we might have considered it also composed of green, red, and violet, as some physicists do). Let I denote the incident white light, and B, Y, R the intensities of blue, yellow, and red necessary to produce white light, so that we may write

$$I=B+Y+R;$$

let there be two solutions containing q and q' of yellow colouring-matter, and let t and t' be the corresponding lengths of columns; then the intensity of the light transmitted through one cylinder will be

$$(1-mqt)\mathrm{Y}+(1-m_1qt)\mathrm{R}+(1-m_1qt)\mathrm{B};$$

m denoting the amount of yellow light absorbed by a unit-layer, and m_1 the amounts of red and blue absorbed by a unit layer. Also the light transmitted by the other cylinder will be

$$(1-mq't')\mathrm{Y}+(1-m_1q't')\mathrm{R}+(1-m_1q't')\mathrm{B}.$$

Since both cylinders are of the same colour, these expressions will be equal. m will be less than m_1 because the transmitted light is yellow. Let $m=m_1-\mu$; then we shall have

$$\mathrm{I}(1-m_1qt)+\mu qt\mathrm{Y}=\mathrm{I}(1-m_1q't')+\mu q't'\mathrm{Y}, \qquad \text{(A)}$$

the expression on the right hand denoting the light transmitted through one cylinder, and the expression on the left hand denoting the light transmitted through the other cylinder. Each expression consists of two terms. The term of the form $\mathrm{I}(1-m_1qt)$ denotes the white light transmitted; the term of the form $\mu qt\mathrm{Y}$ denotes the excess of yellow: this term we may call the effective yellow; for it is the only portion which produces the sensation of colour. Now suppose the light, before passing through the cylinder, to pass through a yellow screen; suppose the composition of the incident light, after transmission through the screen, to be

$$\rho\mathrm{Y}+\rho_1\mathrm{B}+\rho_1\mathrm{R},$$

ρ being greater than ρ_1, say $\rho=\rho_1+r$, so that the composition of the light may be written

$$\rho_1\mathrm{I}+r\mathrm{Y},$$

rY being the effective yellow after passing through the screen. After using the screen, the left-hand expression of (A) would become

$$\mathrm{I}\rho_1(1-m_1qt)+\mathrm{Y}r-\mathrm{Y}rm_1qt+\rho_1qt\mathrm{Y}\mu.$$

Since $qt=q't'$, if we substitute $q't'$ for qt in the last expression, we shall get the light transmitted by the other cylinder after using a screen; hence, if the columns be adjusted so as to produce equality of colour with white light, they will still be in adjustment if the light should become tinged with yellow.

Let y_1 and y_2 denote the effective yellows in one of the cylinders with white incident light and with yellow light; then we shall have

$$y_1=\mu q\mathrm{Y}t,$$

$$y_2=\mathrm{Y}r-\mathrm{Y}rm_1qt+\mathrm{Y}\rho_1\mu qt.$$

Now suppose the length of the column of coloured liquid to be altered a little so as to become $t+\delta t$; let δy_1 and δy_2 denote the alterations in the effective yellow in each case, then

$$\delta y_2=(\mathrm{Y}\rho_1\mu q-\mathrm{Y}rm_1q)\delta t,$$

$$\delta y_1=\mathrm{Y}\mu q\delta t.$$

Hence

$$\delta y_2-\delta y_1=-\{\mathrm{Y}rm_1q+\mu q\mathrm{Y}(1-\rho_1)\}dt.$$

Therefore δy_2 is less than δy_1; that is to say, we shall not see so much difference of colour for a given alteration of depth when the light is tinged with yellow as when it is white; therefore the sensibility of the method is diminished. This may be put in another way. When the incident

light is tinged yellow, the expression for the effective yellow after transmission through the cylinder is

$$Yr - Yqt(rm_1 - \rho_1\mu).$$

Suppose the term in brackets to vanish, then the expression for the effective yellow becomes Yr, which is independent of the quantity of colouring-matter and of the length of the column.

VII. *List of Leguminosæ observed growing near the Egyptian Seashore, West of Rosetta,* 1875 *to* 1877. By H. A. HURST and A. LETOURNEUX.

Read before the Microscopical and Natural-History Section, April 7th, 1879.

[Mar. indicates that the plant is said to grow in Marocco by John Ball, Esq., in his "Spicilegium Floræ Maroccanæ," Journal Linnean Society, 1878. Lx. Exs. shows that the species is represented in the 'Plantæ Ægyptiacæ auspico A. Letourneux lectæ,' under the number given.]

Argyrolobium uniflorum, Decaisn. Jaub. et Spach; Lx. Exs. 41.

Sinai; Southern Palestine; Libanotic Syria; Tunis; Southern Algeria.—*Boiss.*

The occurrence of this plant in Northern Egypt was, I believe, first ascertained by H. H. Calvert, H.B.M. Vice-Consul at Alexandria.

Representative of a Cape genus, it is most difficult of

detection on the sands of the desert, though, when once seen, more easily found. While looking for it, I walked over many specimens, till, when accidentally kneeling on the ground, it attracted my attention.

A. Linnæanum = *Cytisus argenteus*, L., of the northern shores of the Mediterranean, may be known to most of us; but the other species of this Cape genus are seldom seen, except in large collections.

Lupinus termis, Forsk.

Arabicè *Termis.*

Generally cultivated in Egypt; but notwithstanding Boissier's remark "spontaneous in sandy places," I cannot say I have seen it so. Its seeds are edible, which is rather an exception in this genus. They are sown on the muddy banks of the receding Nile broadcast without being covered. The bitter seed becomes sweet during germination, owing to the transformation of its starch into sugar. In this state of germination the seeds have been an article of consumption among all the nations at one time or other subdued by the Arabs.

L. —— ?

Near Mex I found among some barley a fragment which was probably *L. digitatus*, Forsk., but too imperfect to identify.

Genista rætam, Forskohl.

G. monosperma β. rigidula, D.C. Prodr.
Retama rætam, Boissier, F. O.; Forsk.; Lx. Exs. 40; et auctores.

The representative in Eastern Africa of *G. monosperma*

of Western Africa, so well known in our gardens. Of this plant, the first discoverer, Forskohl, says :—" It is the emblem of all that is most miserable in life. Its roots are eaten only by those who can find no other food. It is found in arid sterile deserts destitute of shade. Its branches are few, thin, and scattered." Thrown into the fire, it burns with a loud crackling noise; and seems to be the plant named in Psalm 120, v. 4, as Juniper (*Retem*). If so, it is interesting, as bearing on the Darwinian theory, to find a plant which has retained its name so long, whatever modifications may, unknown to us, have taken place in its structure.

Syria; North-eastern Africa.

The plant found about Alexandria is called *Retama Duriæi* in Letourneux's 'Second Century,' no. 186; but must be Forskohl's plant.

Hooker and Ball, in their 'Tour in Marocco,' say, speaking of *G. monosperma* :—" There is something sad in the meagre and drooping aspect of the plant, that brings to mind those dismal mourning trinkets wherein a lock of hair is made to form the effigy of a Weeping Willow."

Bentham and Hooker retain *Retama,* a genus of Boissier's, among the *Genistæ* (p. 482).

Ball, 'Spicilegium Floræ Maroccanæ,' p. 398, remarks of *G. monosperma,* Lam. :—" fugit loca arida saxosa," the reverse of Forskohl's plant. Are these two the same species under different conditions of life ?

Syria; North-eastern Africa.

Ononis vaginalis, Vahl, i. p. 53; Lx. Exs. 42.

=*O. kotschyana,* Fenzl.

=*O. Cherleri,* Forsk., *non* L.

= *O. vestita,* Viv. Lib.

An interesting and well-marked species, well seen about Ramlé.

Antilebanon (*Gaill.*); Graciosa; Lancerotta (Canary Island); Cyrene (*Boiss.*).

O. reclinata, L.

Canary Islands; Southern Europe; Northern Africa; Cairo; Grecian Islands; Abyssinia; Syria; Palestine; Arabia Petræa.

O. serrata, Forsk.

This species has often been confused with *O. diffusa,* to which it is closely allied.

Canary Islands; northern and southern shores of the Mediterranean; Arabia Petræa; Southern Persia.

O. sicula, Guss.

Only found by me to the westward of Alexandria.

Southern Spain; Sicily; Northern Africa; Arabia Petræa; Palmyra; Aleppo; Southern Persia.

O. mitissima, L.

Rare.

Madeira; Canary Islands; shores of the Mediterranean; Palestine; Mesopotamia.

These four last species occur in Algeria and Marocco (*Munby & J. Ball*).

Trigonella fœnum-græcum, L.

Trigonella fœnum-græcum is a plant which, in former days, was much esteemed. We only know it in England as a condiment to mix with mouldy hay.

Native name *Helva*. The scent very peculiar; said to be used as a condiment in Thorley's Food for Cattle.

In cultivated ground.

I doubt whether this be indigenous. In fact it is so generally cultivated in the East that it would be difficult to say where it is truly spontaneous.

Mediterranean basin; Abyssinia.

T. hamosa, L.; Lx. Exs. 43.

Egypt; Tropical Nubia.

T. laciniata, L.; Lx. Exs. 45.

The plant found about Alexandria, chiefly about Gabari, is the var. β. *subsessilis*, = *T. arguta*, Vis., = *T. nilotica*, Presl.

The typical *T. laciniata* is found near Cairo.

T. maritima, Delile; et var. β. *dura*.

T. dura, Vis.

Sardinia; Sicily; Tunis; Lower Egypt; Joppa.

The luxuriant forms of the typical *T. maritima*, grown near irrigated land, differ so widely from the var. β. *dura* that it is difficult to believe they are the same species; but there seems little doubt of this being the case.

T. anguina, Forsk.; Lx. Exs. 46.

Occurs in the vicinity of Alexandria on irrigated land, but is not common. It is more frequent near Cairo.

Canary Islands; Algerian Sahara; Tunis; Babylonia, near Mohammera (*Noé*).

T. occulta, Del.; Lx. Exs. 44.

T. stellata, of Forsk., is indicated by friends as growing in this district; but I have no specimens: I think it possible it may be a form of *T. hamosa.*

None of the above species is named by Mr. Ball as growing in Marocco.

Medicago marina, L. Mar.

M. littoralis, Rhode. Mar.

M. tribuloides, Desr. Mar.

Mr. Ball seems quite correct in holding that these two are distinct species, although that close observer Lowe has joined them.

M. denticulata, Willd.

M. apiculata, Willd.

M. maculata, Willd. Mar.

M. laciniata, All. Mar.

M. orbicularis, All. Mar.

M. lupulina, L.

M. coronata, Lam.

M. minima, Lam. var. *longiseta*.

Trifolium angustifolium, L. Mar.

T. bicorne, Forsk.

Probably *T. resupinatum*, L.

If *T. bicorne*, Forsk., be really *T. resupinatum*, L., it is the var. *β. minus*, Fl. Orient. ii. p. 137, distinguished as "caulibus tenuioribus sæpe abbreviatis, foliolis minoribus, pedunculis gracilioribus, folio sæpe brevioribus (semper, *Hurst*), capitulis minoribus, dentibus labii superioris calycis fructiferi brevioribus, calycis parte inflata minus elongata," and not the var. *α. majus* of Boissier," caulibus robustioribus, pedunculis folio longioribus, floribus majoribus." This, Will. and Lan. say, is the *T. suaveolens* of Willd.; but I am inclined to think the Egyptian plant is a distinct species from that; therefore Forskohl's *T. bicorne* should stand as such.

T. fragiferum, L. Mar.

T. tomentosum, L.; Lx. Exs. 187. Mar.

T. nigrescens, Viv.

T. alexandrinum, L.

T. formosum, Urv.

Rare. A species of the *Chronosemium* section was found by Mr. Letourneux; but the specimens were mislaid.

Melilotus sulcata, Desf. Mar.

M. messanensis, L.

M. elegans, Salzm.

M. parviflora, Desf., =*indica*, All. Mar.

Hymenocarpus nummularius, DC.; Lx. Exs. 47.

Medicago circinata, var. β, Willd.

Boissier only gives this plant as growing in Egypt and Southern Persia.

Lotus argenteus, Del.; Lx. Exs. 184.

L. edulis, L. Mar.

L. creticus, L. Mar.

L. cystisoides, DC. Mar.

L. corniculatus, L.

L. tenuifolius, Rchb.

L. pusillus, Viv. Fl. Lib.

With the following:—

L. pusillus, var. β. *major*, = *L. halophilus*, et *L. Aucheri*, Boiss.; pedunculus biflorus vel rarius 3–4-florus.

This latter plant may be considered a good type of a variety. In a dry season only the type can be found; while in a season with an excess of wet, such as 1876–7 the variety β predominates on the very same ground.

Sicily; Attica; Syria; eastern part of Northern Africa;

Grecian Islands; Arabia Petræa; Southern Persia, at Buschir.

L. cystisoides, L.; Kotschy, pl. Ægypt. exs. no. 949.

Lotus creticus, *β. cytisoides*, Boiss. F. O. p. 165.

L. creticus, L.; Rchb. Icon. t. 134.

Var. *α. genuinus*, Boiss. F. O. p. 165.

See Prod. Floræ Hispanicæ of Willkomm and Lange, p. 341, for a good discrimination of these two species or varieties. They both occur around Alexandria.

L. ornithopodioides, L.

Scorpiurus subvillosa, L. Mar.

S. sulcata occurs more in the interior; but I have not seen it in this district.

Tetragonolobus palæstinus, Boiss; Lx. Exs. 185.

Sesbania ægyptiaca, Pers. Syn. ii. 316; Lx. Exs. 57.

Æschynomene sesban, L. Roxb.
Æ. indica, Burm.
Coronilla sesban, Willd., Rheede.

Plains from the Himalayas to Ceylon and Siam, ascending to 4000 feet in the north-west; cosmopolitan in tropics of the Old World (*J. G. Baker*, *F. I.*); Senegal; Nubia; Afghanistan and Eastern India (*Boiss. F. O.*).

This plant reminds one of the genus *Coronilla*, under which it is placed by Willd. Boissier, in his 'Flora Orientalis,' asks whether it is really spontaneous in Egypt—a question I must echo. I never saw it looking truly wild. It has probably been introduced from India in very ancient

times, and is, perhaps, the most easily recognized plant in old Prosper Alpinus's work.

Astragalus hispidulus, DC.; Lx. Exs. 51.

A. annularis, Forsk.; Lx. Exs. 52.

A. bæticus, L. Mar.

A. hamosus, L.; Lx. Exs. 189. Mar.

A. hamosus, var. legumine majore, Lx. Exs. 188.

A. hamosus, var. legumine dorso profundius sulcato, Lx. Exs. 49.

These three forms appear worthy of further examination. The latter may be distinct.

A. trimestris.

A. mareoticus, Del.

A. tribuloides, Delile; Lx. Exs. 48.

A. radiatus, Ehrenb.

A. peregrinus, Vahl; Lx. Exs. 53.

A. alexandrinus, Bois. Diag. ser. 1, ix. p. 74; Lx. Exs. 54.

A peculiar and distinct-looking plant, though allied to *A. platyraphis*, Fisch.

A common plant from Alexandria to Aboukir; also found in Arabia Petræa, Palestine (on the Jordan, near Damascus), and Tunis.

A. trigonus, DC.; Lx. Exs. 55.

A. trigonus is the sole representative in this northern district of the woody *Astragali* of which further south so many species occur. They and the Acacias are the sources of our supplies of gum.

Hippocrepis cornigera, Boiss.; Lx. Exs. 56.

H. divaricata, Hochst. MSS.

Its older name is *H. bicontorta*, Loisel. Fl. Gall.

It hardly seems correct for Boissier to substitute his own name for an older one, even on his own ground (nomen antiquius sed improprium).

Northern parts of Eastern Africa; Arabia Petræa; Ramleh, Syria; Southern Persia, at Abuschir.

H. unisiliquosa, L.

Basin of the Mediterranean; Portugal and Spain.

H. multisiliquosa, L.

Southern Europe; Northern Africa.

Both the above species are commoner to the west than the east of Alexandria.

H. biflora, Spreng.; Mex.

Onobrychis crista-galli, Lam. Mar.

O. Gærtneriana, Boiss.

Alhaji maurorum, DC.; Lx. Exs. 58.

Hedysarum alhaji, Linn.
Manna hebraica, Don.
A. mannifera, Desv.

The Camel's Thorn, found from Songaria to Greece and

Nubia. Plains of North-western India, Upper Ganges and Concan, ascending to 3000 feet on the Kishangunga (*J. G. Baker*, F. In.).

Vicia sativa, L. Mar.

V. lutea, L., var. *hirta*, Boissier. Mar.

V. hirta, Balbi et auct.

V. angustifolia, Roth. Mar.

V. angustifolia, var. *a. albiflora*, Boiss.

V. calcarata, Desf.

V. gracilis, Loisel.

Cicer arietinum, L.

Widely cultivated, but origin unknown.

Cicer arietinum is eaten fresh as well as dry, like peas; in the latter state often with the bulbs of *Cyperus esculentus*, when it is called by the natives "Habb el aziz u humus." The word Cicer has attained a melancholy celebrity, as being that by the pronunciation of which the French origin of many of the victims of the Sicilian Vespers was detected. It was the test-word the pronunciation of which decided their life or death.

Ervum lens, L.

Cultivated and escapes.

E. ervilia, L.

Lathyrus aphaca, L. Mar.

L. sativus, L. Mar.

L. cicera, L. Mar.

L. hirsutus, L.

L. marmoratus, Boiss. et Bl.

Indicated in the F. O. as found near Alexandria by Cadet.

Pisum arvense, L.

Parkinsonia aculeata, L.

A native of Tropical America, is subspontaneous in the grounds of the Gabari palace.

Acacia nilotica, Del.

Apparently indigenous and much cultivated on banks of canals; is probably a native of Nubia.

Albizzia lebbek, L., sub *Mimosa*.

Acacia lebbek, Willd.; DC.
Cassia planisiliqua, Burm.

Much planted in squares and along roads, but not a native.

Ad inquirendum :—

Lotonotis.

Coronilla.

Psoralea.

Biserrula.

VIII. *On Colorimetry.*—Part III.

By JAMES BOTTOMLEY, B.A., D.Sc., F.C.S.

Read before the Physical and Mathematical Section, October 14th, 1879.

IN this paper I give the results of some further experiments to test the accuracy of the assumption that, when light is transmitted through transparent coloured solutions, the length of the column multiplied by the quantity of colouring-matter is constant if the colour is constant. In a communication which I made to the Society in April of this year, I gave the results of some experiments with ammonio-sulphate of copper, which appeared to indicate a failure of the law; but the failure was traceable to the decomposition of the salt by water, and better results were obtained when a suitable menstruum was employed. I was wishful to obtain some colouring-matter which might be diluted with water without decomposition; it occurred to me that caramel would be a suitable body. I prepared some caramel by heating loaf sugar. The resulting dark brown vitreous mass dissolved entirely in water. In these experiments I also wished to see if the law would hold when one quantity was a considerable multiple of the other; also the quantities used are no longer mere traces. In order to avoid an ambiguous result from any difference in sensibility to colour of the two eyes, in making the determinations I used one eye only. The cylinders used in these and previous experiments were not specially made for colorimetric purposes. At the bottom they were curved a

little inwards. Measurements were taken from the summit of the curve. When in such cylinders we have short columns of fluid, the depth not being uniform, the colour is not uniform over the whole area as we look through the cylinder at an external white surface. Manifestly the colour at the sides is more intense than at the middle; but for purposes of comparison we must restrict our attention to the middle. It is not easy to confine attention to a limited portion of a coloured area, so as to receive no impression from the remainder of the area, without some provision. Hence it is necessary to limit the field of view at the bottom of the cylinder. This was done either by placing small porcelain disks on a black ground and holding the cylinder so that its axis passed through the centre of the disk, or, still better, by covering the bottom of the cylinder with a black external plate having a small hole (about a quarter of an inch diameter) in its centre. With such a provision, columns seemed in some cases to satisfy the experiment which otherwise would have given the impression of too dark a colour. In these experiments I used a method for determining colours indicated in my last paper, regarding the proper colour as the mean of two sets of determinations, one set giving too great and the other too small values. Thus the determination of colour has some analogy with the method used by old geometers for determining areas bounded by curved lines; considering them as the limits of internal and external polygons. In these experiments A denotes the number of cubic centimetres of caramel solution mixed with water, B the length of the column, and C the number of cubic centimetres thence derived by calculation. In one experiment the mean of four trials for the greater limit gave 2·83 cm.; and the mean of four trials for the smaller limit gave 2·65. Hence the result will be as follows (standard solution contains

10 cub. c. of caramel in 500 cub. c. of water, length of column 21·2 cm.):—

A	B	C
80	2·74	77·4

The above result was obtained by using the right eye alone. I made another series of determinations, using the left eye alone. For the greater limit the mean of four trials was 2·85; and for the smaller limit the mean of four trials gave 2·58. Hence the result will be as follows (standard solution same as last):—

A	B	C
80	2·71	78·2

I next made some experiments with stronger solutions. For the greater limit the mean of four trials gave 2·78, and the mean of four trials for the smaller limit 2·68. Hence the results were as follows (standard solution 40 cub. c. of caramel solution in 500 cub. c. of water; length of column 21·2; observations made with right eye only):—

A	B	C
320	2·73	310·6

I also compared a solution containing 320 cub. c. with another solution containing 10 cub. c.: the theoretical length was 0·65 cm.; a column between 0·6 and 0·7 would satisfy; but the meniscus rendered the exact determination difficult. I also made a further experiment with the solution containing 40 cub. c.; one determination for the greater limit gave 5·7, and one determination for the smaller gave 5. Hence the results are as follows (standard solution 10 cub. c. in 500 cubic c. of water, length of column 21·2 cm., observations made with right eye):—

A	B	C
40	5·35	39·6

Thus the result is very near.

The solutions of caramel ought not to be kept many days. After the lapse of twelve days some of the solutions were turbid and unfit for comparison, owing to the development of vegetable organisms. It seems very probable that even with large differences between the lengths of the columns and with larger quantities of colouring-matter the relation qt=constant is valid when the colour is constant. But suppose the colour to vary, what will be the connexion between the quantity of colouring-matter, the length of the column, and the intensity of colour? If q denote the quantity of colouring-matter per unit of length, and t the total length, we have the relation $qt=c$ if the colour be constant; but if the colour vary, c will be a function of the transmitted light. Hence

$$c=f\ (\mathrm{T})$$

if T denote the transmitted light, therefore $qt=f\ (\mathrm{T})$, or, as we may write it, $\mathrm{T}=\phi(qt)$, the probable form of this function may be obtained as follows:—Suppose we have two perfectly transparent cylinders of unit area and a fluid of such a nature that, if in any portion of it we dissolve some colouring-matter, on further addition of the fluid no decomposition takes place. Suppose we have a standard solution containing one unit of colouring-matter per unit of volume. If the colouring-matter remain constant in quantity, then the intensity of the light will be a function of the length of the column of fluid only, say $\psi(t)$; and if the length of the column of fluid remains constant, the intensity of light will be a function of the quantity of colouring-matter only, say $\phi(q)$. Suppose now that into the cylinders (which we may distinguish as A and B) we pour a unit length of the standard fluid; then the light transmitted will be the same in both; hence we shall have T=

$\psi(1)=\phi(1)$. Dissolve in A another unit of the colouring-matter, and make the column of the standard solution two units long in B; the colour will remain the same; hence we have $\psi(2)=\phi(2)$. If we dissolved three units in A and made B three units long, we should again find $\psi(3)=\phi(3)$, and generally $\psi(n)=\phi(n)$. If, then, we know $\psi(n)$, we shall obtain $\phi(n)$. For the intensity of light transmitted through a column n units long, Sir John Herschel has given an expression (to which I have referred in a previous paper) of the form

$$\Sigma a k^n$$

k being the intensity of light passing through a unit thickness, a the intensity of the incident light, and the summation having reference to the composite nature of light. This formula is given by Herschel in the 'Encyclopædia Metropolitana,' also in an article on the absorption of light by coloured media in the 'Transactions of the Royal Society of Edinburgh.' In neither of these works do I find the experimental confirmation of the formula. It appears to have been obtained *a priori*. If we assume its accuracy we shall obtain for $\phi(n)$ the expression ak^n, if we suppose we are dealing with homogeneous light; if we substitute q for n we shall obtain ak^q for the intensity of light which has passed through a unit length containing q units of colouring-matter. We may now suppose the length to vary: for two units of length the expression will be $a(k^q)^2$ for three $a(k^q)^3$ and for t units $a(k^q)^t$. Finally, if we suppose that there are various kinds of light, we have

$$T=\Sigma a k^{qt}$$

as a probable expression for the intensity of light passing through a column t units long and containing q units of colouring-matter per unit of length. I think that in many

cases where the relation qt=constant fails, it may be traced to some decomposition having taken place, or to some change effected by light. For example, I commenced some experiments with ferricyanide of potassium; but as it did not prove a suitable salt for making experiments without some special precautions with regard to the action of light, I discontinued them. As I am not aware that any one has particularly noticed this darkening, a few remarks may be interesting. A standard solution was prepared containing 0·8 gram in 500 cub. c. In the afternoon having occasion to use this standard for comparison with another, the result was not satisfactory, owing to its transparency not being so perfect as when freshly made. On the following morning I made a fresh standard solution of the same composition; it differed from the old in being more transparent, and I thought that it had more of a greenish tint. This new solution being left on the table before the window, after a time became of diminished transparency; also on looking down into the cylinder a very faint red cloudiness was perceptible. I also compared a solution containing 3·2 grams in 500 cub. c. which had been freshly prepared with a solution containing 6·4 grams in 500 cub. c.; this solution had been prepared on the previous day and had been exposed to light during that interval. I found the length of the column indicated by theory decidedly too great; it occurred to me that the discrepancy was due to some action of light on the ferricyanide. About six o'clock in the afternoon I again compared these solutions; the theoretical length gave a colour which was still too dark, but the disparity of colour was not so marked as at first. The comparison was also disturbed a little by the slightly diminished transparency of the weaker solution.

I now prepared a fresh solution, containing 6·4 grams

in 500 cub. c., thinking that when we work with solutions which vary gradually in colour we are apt to forget the initial condition. This new solution seemed quite different from the old one of the same strength. The latter was much darker and browner. So great was the difference that 9·1 cm. of the old seemed as dark as 22·5 of the new. To find whether the darkening was due to the action of light or to some intrinsic cause, I divided the newly made solution into two equal columns. One I left on the table before the window; the other I kept in a cylinder which was closely invested with black cloth. After the lapse of six hours I compared them. The one exposed was so much darker that 5 cm. of the exposed solution gave a tint as deep as 10·9 cm. of the unexposed. This observation was made on the Saturday. On midday of the following Monday, when I again compared them, the darkening had evidently increased; for 3 cm. of the exposed solution gave a tint about as dark as that furnished by 10·9 cm. of the unexposed.

Wishing to ascertain whether keeping in the dark would reverse the action of light, on Saturday, May 24th, I took a solution containing 6·4 grams in 500 cub. c., the solution having been prepared three days previously and darkened by exposure during that interval to light. The containing cylinder was closely invested with black cloth and kept in a dark closet. On the morning of the following Monday I thought that it appeared not quite so dark as at first; and on the evening of the same day I thought it a little lighter than in the morning. After keeping it in the dark for a week I found that it had become much lighter; and on June 4th, when I examined it again, it seemed nearly as light as a freshly prepared solution; there was, however, a minute quantity of precipitate.

From these results it is evident that in some cases

special provision must be made to avoid needless exposure to light in quantitative determinations by colorimetry, or in studying the laws of the absorption of light passing through coloured solutions.

I also made some experiments with chromate of potash. This I thought a stable salt suitable for experiments. Nevertheless some of the results were not satisfactory when one cylinder contained a solution which was several times stronger than the other. For instance, a standard solution was made containing 0·8 gram in 500 cub. c. of water. Another solution compared with this gave the following results :—

A	B	C
6·4	3·7	4·5865

A repetition of the experiment gave nearly the same result, namely 3·6 for the length of the column.

It occurred to me that possibly, when potassic chromate is diluted, there may be liberated a minute quantity of chromic acid, which would increase its absorbent power; this might be inferred from the greatly increased absorbent power imparted to the bichromate by the additional molecule of CrO_3. I therefore took the cylinder containing the standard solution used in the last experiment, and divided its contents into two equal columns: to one I added a few drops of ammonia; this column became slightly but perceptibly lighter than the other, so that I have little doubt some change had been effected in the constitution of the dissolved salt. The hypothesis of the liberation of a little chromic acid is, I think, strengthened by the fact that a solution of the salt is of a deeper yellow than the undissolved salt. I think that probably a trace of carbonic acid in the water had liberated a little chromic acid.

To try what the effect of the addition of a little weak

acid would be, I took a solution containing 1·6 gram in 500 cub. c. and divided it into two equal parts. To one I added a little extremely dilute sulphuric acid. The colour of this portion became decidedly deeper than that of the other. I also tried what would be the effect of the addition of a little ammonia to a strong solution; so I divided the solution containing 6·4 grms. in 500 cub. c. into two equal portions. One I treated with ammonia: this I thought a little lighter than the other; but the difference was very slight. This, however, we might expect; for any small change of intensity would be less noticeable in a strong solution than in a dilute one.

I now made some fresh experiments with chromate of potash, a little ammonia being added to both columns. The mean of four trials gave for the greater limit 3·35; and the mean of four trials gave for the smaller limit 2·18. Hence the result will be as follows (standard solution 0·8 gram in 500 cub. c. of water; length of column 22·5):—

A	B	C
6·400	2·77	6·498

In this experiment I used the right eye. The theoretical length is 2·81; and the above result is therefore a near approximation.

With the same solutions, on the following day (June 26th) the results were not so favourable; the mean of eight trials gave 2·57 cm. as the length of the equivalent column, the left eye being used in the determinations. On the next day (June 27th) I repeated the experiments with these solutions, using the right eye; the mean of four trials gave 2·3. To each solution I added 5 cub. c. of ammonia, and repeated the experiment; the mean of four trials gave 2·13 as the result. These differences of results are probably due to some internal changes in the coloured fluids.

I may also take this opportunity to correct two numerical errors in the sixth volume of the Society's Memoirs. Page 262, line 3, for 3·2 read 2·2; and on page 264, line 2, for 15 read 50.

IX. *On the Origin of the Word "Chemistry."* By Carl Schorlemmer, F.R.S.

Read November 18th, 1879.

Chemistry as a science is first mentioned* by Julius Maternus Firmicus, a native of Sicily, and procurator under Constantine the Great. He wrote, about A.D. 336, a work on Astrology, which has been preserved only in a defective state, and is commonly known by the name of 'Mathesis.'

In this work he states that by observing the position of the moon, in respect to certain heavenly bodies or constellations, at the hour when a child is born, its future inclinations can be predicted. He continues:—"Et si fuerit haec domus Mercurii, astronomiam. Si Veneris, cantilenas et laetitiam. Si Martis, opus armorum et instrumentorum. Si Jovis, divinum cultum et scientiam in lege. Si Saturni, scientiam alchimiae. Si Solis, providentiam in quadripedibus. Si in Cancro domus sua, scientiam dabit omnium quae exeunt de aqua"†.

Other editions of this work have also "scientia alchimiae"‡; but Vossius informs us that in the manuscripts it

* Kopp, Beiträge zur Geschichte der Chemie, p. 43.

† Julius Firmicus de nativitatibus. Ed. Simon Bevilaqua: Venice, 1497.

‡ Ed. Aldus Manutius, Venice, 1499; Ed. Nicolaus Brucknerus, Bâle, 1533.

is "chimiæ" *. He says:—"Alchimiæ scientiam nominat Firmicus, lib. iii. cap. xv. Ita quidem editum ab Aldo, sed in chirographis est chimiæ."

Athanasius Kircher also states that the manuscript in the library of the Vatican has "chymiæ," and not "alchymiæ" †.

Firmicus does not give any explanation of this term. However, another writer, who probably lived at the same time, if not earlier, explains it. Zosimus, the Panopolite, according to Georgios Synkellos, a writer of the ninth century, states that *χημεία* (or *χυμεία*, as some manuscripts have) meant the art of making gold or silver ‡.

The curious passage in which the word occurs is the following:—

"The sacred Scriptures inform us that there exists a tribe of genii who make use of women. Hermes mentions this circumstance in his Physics; and almost every writing (λόγος), whether sacred (*φανερός*) or apocryphal, states the same thing. The ancient and divine Scriptures inform us that the angels, captivated by women, taught them all the operations of nature. Offence being taken at this, they remained out of heaven, because they had taught mankind all manner of evil, and things which could not be advantageous to their souls. The Scriptures inform us that the giants sprang from their embraces. *Chema* is the first of their traditions respecting these arts. The book itself they called *Chema*; hence the art is called *Chemia*."

It is not difficult to trace the origin of this myth. We find it first in Genesis, chap. vi.: "And it came to pass, when men began to multiply on the face of the earth, and

* Etymologicon linguæ latinæ: Amsterdam, 1695.

† Kopp, *op. cit.* p. 9.

‡ Thomson's History of Chemistry, p. 5.

daughters were born unto them, that the sons of God saw the daughters of men that they were fair, and they took them wives of all which they chose.

"There were giants in the earth in those days; and also after that, when the sons of God came in unto the daughters of men, and they bare children to them, the same became mighty men, which were of old, men of renown."

Alluding to this, later writers state that the fallen angels taught women all the secrets of nature*. That one of these is the art of making gold and silver, however, is first mentioned by Zosimus. Other Greek writers use the word Chemia or Chymia in the same sense; in print we find it first in the Lexicon of Suidas, who lived in the eleventh century, and defines *χημεία* as "the preparation of gold and silver."

All the earlier Greek writers who mention this word were in close connexion with the university of Alexandria; from this it has been inferred that the artificial preparation of the noble metals was first attempted in Egypt.

That country was conquered by the Arabians in 640. Here they made undoubtedly their first acquaintance with chemical science; they prefixed their article to the Greek name, and thus introduced the terms Alchemy, Alchimy, or Alchymy.

The origin and meaning of these terms have often been discussed. Plutarch states that the old name of Egypt was *χημία*, that it was so called on account of its black soil, and that the same word designated the black of the eye. From this the conclusion has been drawn that chemistry originally meant the science of Egypt, or, the black of the eye being the symbol of darkness and mystery, that chemistry was the secret or black art. But alchemy

* Kopp, *op. cit.* p. 4.

has never been called the black art, a name which was exclusively reserved for magic or necromancy.

It has also been stated that the name was derived from the Arabic *kema*, to hide; while others have maintained that the founder of our science was Cham or Ham, the son of Noah, or an Egyptian king with the name of Chemmis. It has further been suggested that the name of the science was derived from *χέω* (to melt), or from *χυμός* (juice or liquid).

To this it has been objected that the original spelling was *χημεία* and not *χυμεία*, which, although Hermann Kopp, the great historian of chemistry, inclines to this view, has not yet been proved satisfactorily. Humboldt believes that the latter word got into some manuscripts by a mistake of the transcriber, and continues:—"Alchimy commenced with the metals and their oxides, and not with the juice of plants." This objection, however, cannot be maintained at all, because vegetable juices, or, at least, substances designated by their names, are mentioned by the older alchemists as the most potent substance by which transmutations could be effected *.

Some time ago my friend Professor Theodores called my attention to an interesting paper on this subject, published by Professor Gildemeister †, in which he maintains the derivation of the word chemistry from *χυμός*. According to him *kîmiyâ* in Arabic does not originally have an abstract meaning, and is the name, not of a science, but of a body by which, or rather by a substance obtained from which, the transmutation of metals is effected; it is synonymous with *iksîr*. Alchemy, as a science, was called the preparation of *kîmiyâ* or *iksîr*, also the science of the preparation of *kîmiyâ*, or, more shortly, science of *kîmiyâ*.

* Kopp, *op. cit.* p. 76.

† Zeitsch. deutsch. morgenländ. Ges. xxx. p. 634.

In the Arabic Lexicon (Qâmûs) *al-iksîr* is explained by *al-kîmiyâ*, and the latter again by the former, or by any medium which, applied to a metal, transports it into the sphere of the sun or the moon, *i. e.* converts it into gold or silver.

Even to this day the word is used in the concrete sense; Kotschy* relates that the pasha of Nicosia talked much of flowers, chiefly *kimia*, a plant having the property of converting metals into gold.

The later writers, however, called the science shortly *al-kîmiyâ*, and retained the term *al-iksîr* (elixir) for the transmuting medium or the philosopher's stone. This latter word is identical with *ξήριον*, as the writers of the Alexandrine school called the philosopher's stone†; while the same name was employed by the physicians for a healing powder used for sprinkling over wounds, *i. e.* a desiccative powder (from *ξηρός*, dry)‡.

Now the correlate to dry is moist or liquid, *χυμός*; and from this is derived *χυμεία*, a moist substance corresponding to *λιθεία*, a material formed of *λίθος*, or *κεραμεία*, the occupation with *κέραμος*.

Ibn Khaldûn, who lived in the 14th century, says that from the philosopher's stone a liquid or a powder might be prepared called *iksîr*, which, when thrown on molten copper converted it into silver, and molten silver into gold. In opposition to its etymology the word is here used for a liquid, because at that time *kîmiyâ* no longer meant the transmuting substance, but the science of transmutation; and this explains why today we may understand by "elixir" a liquid.

* Petermann's Geog. Mitth. viii. p. 294.

† Kopp. *op. cit.* p. 209.

‡ Zosimus calls the substance by which copper is tinged yellow or converted into brass *τὸ διὰ τῆς θουθίας ξήριον*, a powder prepared by means of tutia; now tutia (zinc oxide) is still today used in medicine as a desiccative.

We also find that the philosopher's stone is often called "the red tincture," from *tinguo* (to moisten).

It appears therefore very probable that the name of our science is derived from χυμός; and the proper spelling would therefore be *Chymistry*, as the 'Times' newspaper for a long time insisted. As, however, this derivation has not yet been proved quite satisfactorily, the time-honoured term *Chemistry* will remain in use, and, I think, be retained even if it should be shown that *χυμεία* was the original spelling.

X. *Note on the Identity of the Spectra obtained from the different Allotropic Forms of Carbon.* By ARTHUR SCHUSTER, Ph.D., F.R.S., and H. E. ROSCOE, LL.D., F.R.S.

Read December 2nd, 1879.

SPECTRUM analysis serves as our most delicate test of the chemical constituents of a substance. Hence it appeared not uninteresting carefully to examine the nature of the spectra obtained by the combustion of natural graphite and of diamond in a vacuum of pure oxygen, and to compare the spectra thus obtained with the well-known spectrum of carbonic oxide obtained from charcoal. The preparation of such an oxygen-vacuum which shall yield an oxygen spectrum exhibiting no other lines than those of oxygen is a matter of considerable difficulty. The slightest trace of any impurity containing carbon produces the spectrum of carbonic oxide. For this reason the use of caoutchouc tubing and of greased stopcocks must altogether be avoided, and thus the experimental difficulties are considerably enhanced.

In order to obtain a spectrum of pure oxygen entirely free from the lines of carbonic oxide, a necessary preliminary condition of our experiment, the following arrangement was made. The figure exhibits the form of the tube employed. The part from A to B consists of an ordinary Plücker's tube. At the lower end of this a piece of hard glass tubing (*d*) was sealed. Before the experiment, the requisite quantity of permanganate of potash or oxide of mercury was brought into this, to serve as the source of the oxygen, and then the tube was sealed at the lower end.

The other end of the Plücker's tube was closed by a ground-glass stopper (S), through the sides of which two stout platinum wires (*pp*) were fused, and these were joined together within the tube by a spiral of fine platinum wire (*e*), into which the graphite or the diamond was placed. To prevent leakage between the ground sides of the stopper and those of the tube a drop of mercury, rendered less fluid by the immersion in it of a bit of tin foil, was introduced into the joint. The tube was placed in connexion with the air-pump by means of a side tube sealed on at (C). For this purpose a Sprengel pump was used, to which the side tube was herme-

tically sealed. In this way, and in this way only, was it found possible to obtain a pure spectrum of oxygen. After the connexion with the pump had been made, the whole tube was exhausted, and then the substance contained in the hard glass tube was heated. The oxygen which is given off was then removed by the pump, the tube filled a second time with oxygen, this again removed, and this process repeated over and over again, until at last no other lines but those of oxygen are seen when the spark from an induction-coil passes between the electrodes (*g* and *h*).

When this stage had been reached, and when especially no trace of the carbonic oxide bands could be seen in the tube, the platinum spiral (*e*) containing either the diamond or the graphite was rendered incandescent by means of an electric current.

The spiral contained sometimes a piece of natural graphite, sometimes a Cape diamond; but the result was the same in the two cases. As soon as the platinum spiral had been sufficiently heated, a channelled-space spectrum appeared in the capillary part of the tube. This channelled-space spectrum was carefully compared with the spectrum of carbonic oxide obtained from charcoal and found to be identical with it. No band or line could be seen in the tubes thus prepared which was not also seen in a tube containing carbonic oxide. The spectrum which appears when a Leyden jar is introduced into the circuit is different; but here also we found that every line was due either to oxygen or to carbon. Two lines were seen in the green and greenish yellow which are not contained in any map of the spectrum of carbon or of oxygen, lines which had not been seen in a great many oxygen tubes prepared and examined by one of us. But it was found on further investigation that these are really

oxygen lines, which only appeared at very high temperatures. The capillary portion of the tubes we used was much shorter than that in the ordinary Plücker's tubes, and this accounts for the temperature of the incandescence being higher than usual. As one of the lines is near the unknown aurora line, its wave-length was determined and found to be 5591, showing it to be decidedly less refrangible than the aurora line.

The experiment was repeated in four different tubes and many times in each tube; but whether graphite or diamond was employed, no line was seen which was not also obtained in a tube of the same dimensions containing carbonic oxide.

XI. *On the Anal Respiration of the* Copepoda.

By Marcus M. Hartog, M.A., B.Sc., F.L.S.

Read December 16, 1879.

In a note on *Cyclops* read at the British Association I pointed out that its respiration was exclusively anal. I have now made out the same in *Canthocamptus* (fam. Harpacticidæ) and *Diaptomus* (fam. Calanidæ). In all three the mechanism is the same; at regular intervals, after the backward sway of the intestine, the anal valves open for an instant and then close, giving just time for a slight indraught of water after the opening, a slight expulsion at the close. The necessary pressure to confine the animal seems to interfere somewhat with these move-

ments, sometimes stopping them if excessive; hence I refrain from noting with illusory exactness the intervals between each respiratory movement.

It is to be noticed that the rectum contains, as a rule, liquid only, the bolus of fæces remaining in it but a short time. By endosmose the liquid in the rectum will tend to be at the same condition of gaseous saturation as the body fluid around it, kept constantly agitated by the backward-and-forward sway of the stomach. During the short interval that the anus is open an approach to gaseous equilibrium with the external water takes place, even despite the very slight movement of the water (shown by the little change of place undergone by suspended indigo or carmine particles). In the absence of any other suitable respiratory apparatus, no one can hesitate as to the function of the action I have described.

In the *Nauplius* larvæ of *Cyclops* and *Diaptomus* the working is slightly different. The rectum is a subspherical muscular sac, which at regular intervals contracts so as to leave a linear cavity (along the long axis of the animal), and immediately dilates, sucking up the water from without.

An anal respiration, such as that of *Cyclops*, is found widely among Crustacea—even those which have well-developed gills like *Astacus*, which is one of the highest forms. It has been demonstrated in Phyllopoda and Cladocera, and is here probably the exclusive mode in *Leptodora*, as shown by Weismann. That it is therefore primitive, and should be expected to occur in the primitive or at least very generalized group of the Copepoda, is an obvious deduction. Hence I anticipate that the homœomorphic *Zoea* larvæ of the Decapoda will prove to have this same mode of respiration.

If there be any connexion between Rotifers and *Nau-*

plius, it is easy to make out the origin of the arrangement in the latter. The ciliated funnels and lateral canals of the former can only be of service when there is a thin unchitinized anterior surface through which water can transude into the cœlom: by the extension of chitinization over the whole surface these organs lose their function and abort, while the cloacal "contractile vesicle" takes on an inspiratory as well as an expiratory function, and becomes more or less confounded with the rectum, from which probably, even in Rotifers, it takes origin.

Here must be noticed the wide diffusion of anal respiration in aquatic insect larvæ (alternate inspiration and expiration by the pumping movements of the rectum). This would point to a common origin with Crustacea.

A list of the groups in which anal respiration is made out may be added.

VERMES:

- Rotifera.
- Gephyrea.
- Oligochæta Limicola.

ECHINODERMATA:

- Holothuroidea.

ARTHROPODA:

- Crustacea (general).
- Insecta (most aquatic larvæ).

XII. *The Radiograph.* By D. Winstanley, F.R.A.S.

Read January 13th, 1880.

I have described already one of the several arrangements which I have devised for the automatic registration of the solar radiance*. The instrument in question places a lead pencil on a sheet of paper and writes down therewith when and for how long the sun may chance to shine, but it makes no record of the intensity of his rays. I will now ask your attention to the description of another and much more perfect apparatus, one which continuously records the intensity of thermal radiation to which it is exposed. This instrument I have called the "radiograph." It consists essentially as follows:—A differential thermometer of which the stem is circularly curved is mounted concentrically upon a wheel of brass in a groove cut with that object for its end. This wheel is supported with its plane in a perpendicular position by a knife-edge of hardened steel, which passes through its geometric centre and rests on agate planes. The tube of the thermometer is partly filled with mercury (preferably through half its curve), and, for the reason given in my description of the simple sunshine-recorder, to which I have alluded, a little sulphuric acid is introduced as well. If we now arrange it that the centre of gravity of the solid portion of the system here described shall be below the surface of the planes on which it turns (and the apparatus is provided with adjustments by means of which the point in question can be moved) it is clear that the arrangement may be made to swing in

* Proceedings of Manchester Lit. and Phil. Soc., Nov. 18th, 1879.

pendulous oscillations, notwithstanding the presence of the liquids it contains; for these remain substantially at rest whilst the tube which holds them does, in fact, slide over them (and with very little friction too) in swinging to and fro through arcs of the circle of which its parts are curves. Both bulbs of the thermometer are closed. It is obvious therefore that the tension of the air or gas which they contain will be uninfluenced by the barometric variations of the outer air, the temperature of which latter being experienced equally in each bulb will also leave the equilibrium of the apparatus undisturbed. When, however, one bulb is more heated than the other, the air contained therein will press more strongly on the heavy liquid piston in the tube and wheel the swinging portion of the system round until a fresh position of equilibrium is gained, and this will be (providing that the centre of gravity of the system has previously been made coincident with the point on which it turns) when the tension of the gases is equal in both bulbs. In fact, in so far as now described, the instrument is a differential thermometer, and is that alone—differing in this from Leslie's, that it is a solid and accessible portion of the thing which moves and not the liquid it contains. When, however, one of the bulbs is blackened and the other one is silvered or left clear, the apparatus becomes a "radiometer" in the proper meaning of the term*—that is to say, a measurer of the thermal radiance to which it is exposed and the intensity of which it indicates by variations in the angular positions of a needle prolonged from one or other of the radii of the wheel.

It is only needful now so to arrange it that this needle

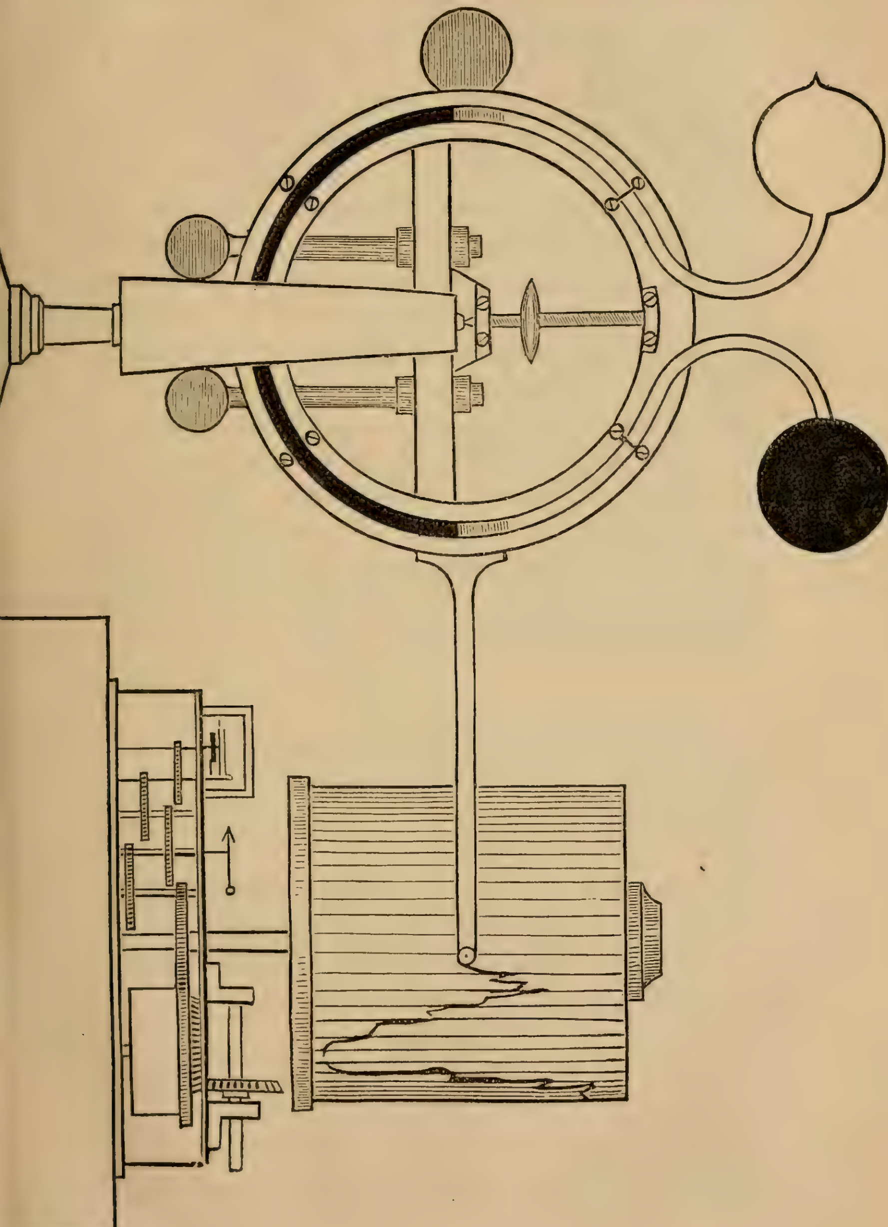
* The "radiometer" of Crookes should in its simple form have been called a "radioscope," as it merely makes visible the effects of radiance, but does not measure their amount.

shall make a tracing of its curves on a cylinder driven by clockwork at an even speed, and the "radiograph" is complete.

Concerning the actual instrument I use, its wheel is -·3 inches in diameter, and the weight thereof a trifle more than two pounds. The other portions of the apparatus are of the same dimensional proportions as are indicated in the sketch. Of course some delicate method of recording has to be employed, and I have thus far used the smoked-paper process so much adopted in the observatories of France. In this way the "radiograms" which illustrate this paper were obtained.

When using the instrument to record the radiance of the sun, I have hitherto exposed it in a box of copper surmounted by a dome of glass into which the bulbs of the thermometer project. The line which joins them is in the plane of the meridian of the place and the black bulb to the north. The box itself is supported, at an elevation of four feet or thereabouts, upon a stand of wood, the legs of which are fimly embedded in the ground. The stand itself is located at the extremity of a garden which overlooks a valley and the sea. A small window in the box permits the movements of the train to be seen and the promptness with which the apparatus acts to be observed. If a cloud "no bigger than a man's hand," and "light as a feather" in its texture, floats before the sun, and occupies but three or four seconds in its transit, its presence, the duration of its passage, and the degree of thermal obscuration it effects are at once set down.

The cylinder of the radiograph passes over a space of ·875 of an inch per hour, a somewhat open range; but, as will be seen on reference to the tracings, the needle often moves for some considerable distance in both directions along the same thin line, thereby showing a practical



instantaneity of action under very ordinary thermal changes in the radiance from the sky. The influence of the sun's rays at daybreak is almost always shown, for some minutes at any rate, before the sun is seen, and occasionally, it would seem, even for hours before his time to rise.

It is not, however, now my purpose to dwell upon the interesting changes which take place in the intensity of the thermal radiance from the sky, my present object being to describe an instrument by means of which they may be recorded or observed. Doubtless in several of its details the "radiograph" may be improved, notably in the condition of its bulbs, and it would unquestionably be better if it computed for itself the areas included by its curves. This, I dare say, I shall presently enable it to do. Meanwhile, as a recorder of the duration and intensity of radiant heat, the instrument, so far as I have seen, is the only one whose readings are uninfluenced by the temperature or the pressure of the air.

XIII. *On an Extension of the ordinary Logic, connecting it with the Logic of Relatives.* By JOSEPH JOHN MURPHY, F.G.S. Communicated by the Rev. ROBERT HARLEY, F.R.S.

Read October 7th, 1879.

THE present paper has been suggested by the section on Elementary Relatives in Prof. Pierce's "Description of a

Notation for the Logic of Relatives," extracted from the Memoirs of the American Academy, vol. ix.

In applying algebraic notation to the ordinary logic, in however extended and generalized a form, the terms denote objects and classes of objects; there is no need of terms denoting relations between these; and the ordinary logic, as generalized and extended by Boole and Jevons, has consequently been called the logic of absolute terms. Nevertheless all logic belongs to the logic of relatives. The logic of relatives is related to the ordinary logic, not as relative is related to absolute, but as the entire science is related to its simplest part. It is in accordance with all analogies drawn from the history of science, that the simplest part of a science should thus be studied alone and brought to comparative perfection, before any one suspects that its problems are not isolated, but constitute only the simplest part of an infinitely wider subject.

From the present point of view, the old logic is defined as that part of logical science which deals with the relations of inclusion and exclusion.

In order to exhibit inclusion as a particular case of relation, we shall need a symbol for it. De Morgan uses L as the symbol of relation generally, and I propose to use *L* in this sense, keeping the Roman capitals for absolute terms.

Combination in logic is analogous, though not closely so, to multiplication (see Boole and Jevons, *passim*). What is the corresponding mathematical analogue of logical relation? The Rev. Robert Harley maintains that it is function generally (see the British Association Transactions, 1866 and 1870); and I have no doubt he is right. But within the very limited scope of the present inquiry it will not be misleading if we treat relation as analogous, though not closely so, to ratio, and the sym-

bols of relation, consequently, as analogous to numerical coefficients.

In mathematics, if any one of the following four equations is true, the rest are true:—

$$\frac{A}{B}=L; \qquad A=LB;$$

$$\frac{B}{A}=L^{-1}; \qquad B=L^{-1}A.$$

The same is true in logic if A and B are understood to be the names of any two individuals or classes, L the relation of A to B, and L^{-1} the inverse relation of B to A. Let L, for instance, be the relation of teacher, then the foregoing equations are thus interpreted:—

The relation of A to B is that of teacher.	A is the teacher of B.
The relation of B to A is that of pupil.	B is the pupil of A.

The equivalence of these four forms has the same kind of self-evidence as the principle of identity and contradiction.

It is to be observed that we use the copula = with the meaning of the word *is*, without raising the question whether there may not be many individuals standing in the relation L to B, and many standing in the relation L^{-1} to A. If it is required that the equation

$$A=LB$$

shall mean that A is the only teacher of B, then the assertion that A is one of the teachers, or a teacher of B, without implying whether B has other teachers or not, will be expressed by

$$A=ALB,$$

and its converse by

$$B=BL^{-1}A.$$

If A and B are both pupils of M, their relation is that of fellow pupils of M. This relation is expressed by

$$\frac{LM}{LM}$$

equivalent in arithmetic to

$$(LM)^\circ,$$

which expression we shall adopt. In arithmetic, every term with zero index has the value of unity, so that if

$$\frac{\mathrm{A}}{\mathrm{B}} = (LM)^\circ$$

we may eliminate, or drop, M, and write

$$\frac{\mathrm{A}}{\mathrm{B}} = L^\circ.$$

In logic we may eliminate in the same way; that is to say, if A and B are fellow pupils of M, they are fellow pupils. But it is not true in logic that all terms with zero index have the same value; and from

$$\frac{\mathrm{A}}{\mathrm{B}} = L^\circ$$

we cannot infer that

$$\frac{\mathrm{A}}{\mathrm{B}} = (LM)^\circ.$$

We may drop at pleasure the absolute term M, but we cannot insert or substitute a term, nor can we drop the relative term L. This is analogous to the rule that in the logic of absolute terms, if

$$\mathrm{A} = \mathrm{ABC},$$

it follows that

$$\mathrm{A} = \mathrm{AB} \text{ and } \mathrm{A} = \mathrm{AC}.$$

But though we can thus drop a factor we cannot insert or substitute one; from

$$\mathrm{A} = \mathrm{AB}$$

we cannot infer either

$$A = ABC,$$

or

$$A = AC.$$

But though in logic a relative term with zero index is not necessarily equal to unity, yet every such term has two important properties which belong to unity and are not combined in any other number—namely, that it is equal to its own reciprocal, and equal to its own second power. Thus if

$$A = L^{\circ}B,$$

it follows that

$$B = L^{\circ}A.$$

That is to say, if A is a fellow pupil of B, then B is a fellow pupil of A. And if

$$A = L^{\circ}M \text{ and } M = L^{\circ}B$$

it follows that

$$A = (L^{\circ})^{2}B = L^{\circ}B.$$

That is to say, if A is a fellow pupil of B and B of C, then A is a fellow pupil of C. This inference is a syllogism, the middle term, M, being eliminated. It must be observed that its validity depends on the relation of fellow pupils being understood in relation to the same teacher throughout. With this convention, the axiom that "fellow pupils of the same teacher are fellow pupils of each other" has the same self-evidence as the axiom that "equals of the same thing are equals of each other;" and both are cases of Jevons's principle of the substitution of similars, that "what is true of any thing is true of its like."

We now proceed to apply these principles to the old logic. The proposition of the old logic, "all A is B," is

expressed in our system by "A is included in B." Using L as the symbol of inclusion, we write it

$$A = LB.$$

Its converse is "B is included in A;" and this we express by

$$B = L^{-1}A.$$

In the old logic,

All A is B

becomes, by conversion,

Some B is A.

But this is an inadequate and indeed an inaccurate account of the subject, because, when reconverted,

Some B is A

becomes only

Some A is B;

so that by reconversion we do not get back the original proposition, which by any satisfactory theory of conversion we ought to do.

We postulate that

$$L^2 = L.$$

This is not true of all relations, but it is true of many, and among others of inclusion. When asserted of that relation, it means that if A is included in M, and M in B, then A is included in B; or, more briefly, the enclosure of an enclosure is an enclosure. This is the expression in our system of the canon of the old "syllogism in Barbara." And conversely,

$$(L^{-1})^2 = L^{-1};$$

that is to say, the includent of an includent is an includent. These words, enclosure and includent, will be found useful.

From the premises

$$A = L^{-1}M, \qquad B = L^{-1}M,$$

we can, as has been shown, infer that

$$\frac{A}{B} = \frac{L^{-1}M}{L^{-1}M} = (L^{-1}M)^{\circ},$$

whence

$$A = (L^{-1}M)^{\circ}B = (L^{-1})^{\circ}B.$$

If L means inclusion, then the meaning of the syllogism is as follows:—

A is an includent of M;
B is an includent of M;
therefore A is a co-includent with B of M;
or A is a co-includent with B.

This is no more than the old logic would express by the following:—

M is A;
M is B;
therefore some A is B.

But if the premises are the converse of these, as follows—

$$A = LM, \qquad B = LM,$$

the conclusion will be

$$A = L^{\circ}B;$$

that is to say, A is a co-enclosure with B—a conclusion not recognized in the old logic; yet it is valid, and may be important. Let E, for instance, mean Irishmen, W the Duke of Wellington, and P Lord Palmerston; then from the premises

$$W = LE, \qquad P = LE,$$

we have the conclusion

$$P = (LE)^{\circ}W = L^{\circ}W;$$

that is to say, Lord Palmerston was a fellow Irishman, and therefore a fellow countryman, of the Duke of Wellington.

As we have seen, out of the relation of inclusion three others arise. We express the four as follows :—

$A=LB$,	A is an enclosure of B.
$A=L^{-1}B$,	A is an includent of B.
$A=L^{\circ}B$,	A is a co-enclosure of B.
$A=(L^{-1})^{\circ}B$,	A is a co-includent of B.

It must be remembered that when we speak of co-inclusion or co-includence, we mean, throughout, inclusion in the same includent, or includence of the same enclosure. This is different from the usage of the common logic. Where the old logicians say

Some A is M,
Some B is M,

we express this by

$\frac{A}{M}=(L^{-1}P)^{\circ}$, *or* A and M are co-includents of P ;

$\frac{B}{M}=(L^{-1}Q)^{\circ}$, *or* B and M are co-includents of Q,

where P and Q are or may be different; and from these premises no conclusion can be drawn.

By combining the four forms of proposition stated above, we get sixteen syllogisms, which constitute as many syllogistic canons. Fourteen of these are conclusive; that is to say, in fourteen cases the relations expressed in the two premises combine in the conclusion into a simple relation, which is always of the same kind with one of the four forms given in the premises; in the remaining two cases they do not so combine. These sixteen are given in the following tabular statement. As the equations

$$L^2=L \qquad \text{and} \qquad (L^{-1})^2=L^{-1}$$

are not generally true of numbers, the conclusions of these syllogisms are not all true in arithmetic, except when L has the value of unity, of which the interpretation is that the enclosure is co-extensive with its includent. Those conclusive forms are doubly underlined which are true in arithmetic for all values of L; those are singly underlined which are true in arithmetic only when L has the value of unity; and the inconclusive ones are not underlined. It will be seen that eight out of the sixteen are doubly underlined.

1.	$\underline{L . L = L}$	9.	$\underline{\underline{L^0 . L = L}}$
2.	$\underline{\underline{L . L^{-1} = L^0}}$	10.	$\underline{L^0 . L^{-1} = L^0}$
3.	$\underline{L . L^0 = L^0}$	11.	$\underline{\underline{L^0 . L^0 = L^0}}$
4.	$\underline{\underline{L . (L^{-1})^0 = L}}$	12.	$L^0 . (L^{-1})^0 = L^0 . (L^{-1})^0$
5.	$\underline{\underline{L^{-1} . L = (L^{-1})^0}}$	13.	$\underline{(L^{-1})^0 . L = (L^{-1})^0}$
6.	$\underline{L^{-1} . L^{-1} = L^{-1}}$	14.	$\underline{\underline{(L^{-1})^0 . L^{-1} = L^{-1}}}$
7.	$\underline{\underline{L^{-1} . L^0 = L^{-1}}}$	15.	$(L^{-1})^0 . L^0 = (L^{-1})^0 . L^0$
8.	$\underline{L^{-1} . (L^{-1})^0 = (L^{-1})^0}$	16.	$\underline{\underline{(L^{-1})^0 . (L^{-1})^0 = (L^{-1})^0}}$

The truth and the applicability of the fundamental equation of this system, namely

$$L^2 = L,$$

do not depend on the interpretation of L as inclusion; this equation is merely the expression of the transitiveness

of the relation; and if L is understood to mean any transitive relation whatever, all the syllogisms as stated above remain true, but with changed interpretations. Let L, for instance, mean cause, then L^{-1} will mean effect, L° concause (to use a good though obsolete word), and $(L^{-1})^{\circ}$ co-effect; and let us assume that the cause of a cause is a cause; or let

L	mean	superior,
L^{-1}	,,	inferior,
L°	,,	co-superior,
$(L^{-1})^{\circ}$	,,	co-inferior;

with either of these interpretations of our symbols, all the sixteen syllogisms will remain true.

The interpretations of the foregoing sixteen equations are as follows:—

1. Enclosure of enclosure is enclosure.
2. Enclosure of includent is co-enclosure.
3. Enclosure of co-enclosure is co-enclosure.
4. Enclosure of co-includent is enclosure.

5. Includent of enclosure is co-includent.
6. Includent of includent is includent.
7. Includent of co-enclosure is includent.
8. Includent of co-includent is co-includent.

9. Co-enclosure of enclosure is enclosure.
10. Co-enclosure of includent is co-enclosure.
11. Co-enclosure of co-enclosure is co-enclosure.
12. Co-enclosure of co-includent constitutes no relation.

13. Co-includent of enclosure is co-includent.
14. Co-includent of includent is includent.
15. Co-includent of co-enclosure constitutes no relation.
16. Co-includent of co-includent is co-includent.

We have now to speak of the relation of exclusion, which is expressed in the common logic by

A is not B;

whereof the inverse is

B is not A.

If we use N as the symbol of exclusion, the above proposition will be expressed by

$$A = NB,$$

and

$$B = NA.$$

This relation is thus seen to be invertible; or, to express it symbolically,

$$N^{-1} = N,$$

an equation which is true of two numbers, namely 1 and -1. Further, N is not equal to its own second power; so that it combines these two characters, which are united in negative unity and not in any other number, that it is not equal to its own square, and is equal to its own reciprocal. Consequently the following equations are true in arithmetic as well as in logic,—N meaning in logic excludent, and in arithmetic negative unity; N° meaning in logic co-excludent, and in arithmetic unity, like any other term with zero index:—

$$N . N = N^{\circ}; \qquad N . N^{\circ} = N;$$
$$N^{\circ} . N = N; \qquad N^{\circ} . N^{\circ} = N^{\circ}.$$

The following are the logical interpretations:—

Excludent of excludent is co-excludent.	Excludent of co-excludent is excludent.
Co-excludent of excludent is excludent.	Co-excludent of excludent is co-excludent.

In this system, when we speak of exclusion and co-

exclusion, we imply that it is the same thing which is excluded.

The foregoing logical equations will be equally true if we interpret *N* to mean "not related to, either as cause or as effect," or "out of relation with."

I believe I have in this paper shown an unexpectedly direct transition from the common logic to the logic of relatives.

XIV. *The Word "Chemia" or "Chemistry."* By R. Angus Smith, Ph.D., F.R.S., &c.

Read March 23rd, 1880.

The meaning of the word "chemistry" has been discussed by several writers recently, as well as by many in early times. The observations made by Professor Schorlemmer and the paper by Mr. Mactear (one at the Literary and Philosophical Society of Manchester, and the other in Glasgow) have caused me to return to the subject, which I had at various times thought of. I have always considered the book of Borrichius 'De Ortu et Progressu Chemiæ' (on the rise and progress of chemistry) to have given the right direction of thought; and the Greek manuscripts on the subject, at least on the sacred art, point so strongly to Egypt by the use of the words Isis and Osiris as symbols of their mysteries that there seemed little room for doubt.

I shall begin as if all my readers were acquainted with the history from the 4th or 5th century according to the

work 'Geschichte der Chemie,' by Prof. Kopp—who must be regarded as the writer of the most complete history, although every one who wished to obtain the fullest knowledge would read also Hoefer's 'Histoire de la Chimie.'

If Firmicus used the word *chemia* for "chemistry" so early as the fourth century, it is clearly not an Arabic word. We find in the 'Beiträge zur Geschichte der Chemie,' by Prof. Kopp, that it is a word not well defined by him, although from later (still not much later) writers we find that it refers in the same century to gold or metals. Later still, possibly not also at a very early time, the transforming of metals was the absorbing idea. The use of the word went also, as we know, towards the meaning "elixir of life." The known tract by Democritus speaks of many chemical subjects, whilst metals and purple dyeing are said to have been treated by him in works not extant; and he is, by legend at least, said to have learnt the wisdom of the Egyptians as well as of the further East. We may take Kopp's period for him as being correct, viz. the fourth century. Everywhere, as we go back, we have our eyes turned to Egypt or Assyria.

The reason, however, for connecting Chemistry and Egypt can have no sound relation whatever to Plutarch's assertion that Egypt was called *Chem* because of the black soil. Neither has it to do with the "black art," as Dr. Schorlemmer truly remarks—a name never connected with chemistry, although we must remember that some (and one may say many) individuals have combined in themselves the magician and the chemist. The expression "black art" is not of extreme antiquity, so far as I know. I was strongly inclined to doubt the accuracy of Plutarch, although I have not been in Egypt, and to say that the soil is not black, and to think that the people were meant to be alluded to. I have some of the mud of the Nile, brought me by a friend; and as

that covers the whole of Egypt, I thought myself justified. Besides, from the drawings of Egypt, which are very numerous, we obtain no idea of blackness. However, I find that Brugsch's wonderfully interesting book 'Egypt under the Pharoahs'* puts the expression in a form which is very clear, as well as convincing up to a certain point. He says that "on countless occasions the King is mentioned in the inscriptions as the 'lord of the black country and of the red,'" thus distinguishing the fertile land from the desert. He also adds "that the Egyptians designated themselves simply as the people of the black land, and that the inscriptions, so far as we know, have handed down to us no other appellation as the distinctive name of the Egyptian people" (vol. i. p. 10). We must conclude then, I would say, that *ham* does not always mean "black," but sometimes simply "dark" in comparison with the sand.

Information like this, confirming (although with some modification) opinions drawn from the earliest times, leads us into difficulties in arriving at the real meaning of "chemistry." It is certainly true that the name of an art might be derived from a country; we have an instance in the word "japan," used for a varnish or lacquer of a superior kind; and that the name of objects made in a country may take the name of the country itself, we have a proof in the word "china," used for porcelain. So people finding an art coming from Egypt, and having no native name at hand for it, might have given it the name of the country; but so many things came to the north of the Mediterranean from that country on the Nile that it was most certainly not the habit of Greeks and Romans to adopt such a mode of speech, as it would have produced great confusion. Of course we might add that one name at least

* A History of Egypt under the Pharoahs, derived entirely from the Monuments. By Brugsch-Bey. (Murray: 1879.)

might be taken from the name of the country, one prominent art for example. This is fair; but where was chemistry such a prominent art or branch of thought or practice that it should be so honoured as to receive the name of this wonderful country?

Whilst, then, we are driven by the earlier arguments to look to Egypt as a source of the art, we are repelled from the belief that the name of the art came from the name given to Egypt as a whole; and we are prevented also from believing that any word signifying "black earth" has turned itself round to mean the science of chemistry, although transformations as great are by no means uncommon, or we might rather say are the rule, in all languages.

I lately came upon a new line of thought, which pleased me much. The Hebrew word חֵמָה (*Hema* or *Khema*) means "heat;" and in one dictionary the meaning of "poison" is also given. This seemed to suit perfectly. But Professor Theodores tells me that when the word is used in the Bible in connexion with serpents it means, more correctly, their violence than their poison; and this is connected with heat and glow. Now, if there is any natural action more connected with chemistry than another, it is that which arises from heat; and it is especially the case whenever metals are extracted from their ores or much manipulated. In all probability the first use of chemistry was for obtaining metals from their ores. But we do not require to keep to the furnace-work only, in order to obtain a good idea of the early chemistry. The Egyptians were chemists in the practice of pharmacy to a very large extent; that is, we know that they made preparations from plants for the cure of diseases and wounds, and that they made very effective preparations for disinfection. They divided gold finely enough for stuffing teeth; but I suppose it must have been used as leaf; at least they

could make it fine. They seem also to have dissolved metals for marking-ink, showing that they used acids. As to distillation (which used to be thought such a late invention) the oil of cedar, used by the embalmers and mentioned by Herodotus, points distinctly to that process. Soda was used; and it was made caustic by lime, as Pliny tells us, and exported to Gaul. This is a peculiarly chemical process.

Judging from these facts, is it possible for us to consider that the arts employing all these operations, for each of which heat was used, could possibly have escaped having a name? Prof. Kopp does not, so far as I see, feel sure that a name existed early; and others have had similar doubts, as we have seen, whilst there has been a strong inclination to derive the word from some simple object or fact. Such an origin for a word is probably more usual in the history of language than any other; and it is not to be expected that the difference between abstract and concrete words should have received much consideration in Egypt, although not unknown, since we can point to a distinct recognition of it. There is a manuscript which is considered by Dr. Birch to have come from 1100 B.C., which has the following title:—"Principle of Arriving at the Knowledge of Quantities, and of Solving all Secrets which are in the Nature of Things." It is arithmetical and geometrical. This I have obtained from Mahaffy's 'Prolegomena of Ancient History;' but I understand that we may soon expect much more information on such points from manuscripts now under examination. The medical treatises, as well as the anatomical, show abundant abstract thinking.

From these considerations I see no difficulty in supposing that the Egyptians used one word to designate the strange phenomena produced in substances by means of heat. If we consider the operations of a laboratory, we find it re-

markable that heat comes into nearly all of them, and heat to a large extent: there are exceptions, especially those peculiarly modern actions producing changes in solutions. It is an old saying that bodies do not act but when dissolved; and in order to dissolve them, fuse them, or make them liquid, it is seldom that heat to a very sensible amount does not take part in the process.

Did not some old Egyptian see some one working with furnaces at the mines of Midian, at Akita, east of Coptos, or any places between the Nile and the Red Sea, bringing out lead and silver, and collecting small amounts of gold at the bottom of a crucible? and did he not speculate on the wonderful things produced by heat in the chief towns in Chem of the Thebaid? When he went over to Ramses or to Heliopolis, we can suppose him following his studies among the physicians, who had most certainly laboratories in which they boiled their plants and roots and seeds. They could not boil without vessels in which to boil them; and wood or charcoal fires under the vessels were necessary; and that there were roofs to cover the workers is most probable, if not certain, whilst men would stand and watch the processes. These are chemical operations; and there is a similarity in the appearance to an outsider amongst them all. If, too, we include the dissolving of metals and the boiling down of soda solutions, the idea is widened. And we can suppose the same inquirer going to a dye-house at Tini, on the west of the Nile, and observing the cloth treated with different mordants plunged into the boiling dye solution, perhaps madder, and coming out of various colours, even when only one colour was used. It seems to me, then, not likely that such a class of artisans and professional men would be without a comprehensive name for their labours. A name which did emerge out of darkness into near places is known to be "chemia;" and this is the word so much

criticised. If we can prove it to be associated with or to mean "*heat*," it is more nearly associated with such arts than any we know. But the people who obtained the word in Europe did not obtain the arts, except in a very imperfect manner; and thus, when the meaning was sought, it was given also in that imperfect manner which ignorant people are driven to. Thus many have imagined that it referred only to gold-making; but we are not called upon to take their definition of the word, as no early writer has considered the subject from a wide view.

I say, when the word came out of darkness; with this I mean that when it came to be used in the West, it came with all its connexions traced to Egypt. Nay, the first reputed writer on subjects distinctly chemical comes from Egypt. It may be said that we are not sure of his history; still it is also true that he has no claim to come from any other place, and there seems no reason to deny the connexion. I refer to Zosimus. He might be enough, had we no other writings having a frequent reference to Egypt; and although these may in some (not quite in all) cases have been written in Christian periods, there seems no reason to suppose that the writers were mistaken in attributing the origin of the ideas to Egypt. The Egyptian system was slowly broken up, and traces of its knowledge were scattered in miserable fragments among writers, although artisans may, in a traditional way peculiar to themselves, have retained many useful inventions.

While this is a fair view of a possible case, I do not think it probable that the word came directly from "heat" by one act of the mind of any Egyptian or other man. It came, as many words do, by a tortuous route, but keeping, as it were, in view its true origin by a constant association with things and places connected with heat.

It appeared to me that the Hebrew was not the

language from which to find the root of the word; but it was necessary to work out the idea begun; and for this reason I took the opportunity of the nearness and friendliness of Professor Theodores, from whom also I sought guidance in going into other languages of the East.

Having been satisfied that *Ham*, "black" and "secret," could have no useful meaning as applied to chemistry, I decided to search if my new idea of chemistry as an agent or art depending on heat was well founded, knowing next to nothing of Hebrew, although three brothers who knew more or less of Eastern languages gave me once a taste for it; I found that the word which I may write *Hama* meant not only heat but the sun. Knowing also that the vowels in the word were, under certain circumstances, changeable, it was not difficult to reach *Hem*; and we may say that every one knows that the name of Egypt is *Ham*, or *Khem*, or *Khemi*.

I then went to Prof. Theodores, fearing to trust myself in the maze of Hebrew etymologies; and he gave, as the meaning of the most important derivative of *Ham*, "burnt black," and quoted from Littré's Dictionary under the word *Chimie*, p. 76, "*Cham, Kem, Khemi* is a name often read on the hieroglyphic monuments; it signifies properly 'black earth,' and is the name of Egypt." It seems to me that the meaning of an important derivative being "burnt black" is a most valuable step in advance. This seems the proper mode of connecting the word with *heat*. We can scarcely imagine the word *black* being the root of the word *sun*; but we can easily imagine the word *heat* being so; and the heat, burning, leads us readily to *black*.

Prof. Theodores says, "חמם (cognate in character to יחם) means to be hot, to heat; from it are derived the nouns *Ham-mah*, 'heat,' also 'sun,' the receptacle of heat. Another noun is *Hema*, 'fiery anger.' This latter form

occurs in Deuteronomy, xxxii. 24, in connexion with the names of snakes or some such animals."

"The fundamental meaning of the Arabic root *Hamma* is also 'to be hot;' but this verb has been made to serve for 'to decree,' 'to be black,' 'to have a fever.'" After giving a number of derivatives, he says, "Many of the above words (*Hummums,* for instance) have been adopted over all Asia by non-Semitics no less than Semitics." He considers the root to be Egyptian.

Prof. Theodores says that, "if *Kimia* be an Arabic word, the attempt to graft it on *hot* or *black* is futile." This being from grammatical considerations, I take it as decisive, as I will not venture on transcribing the Arabic vocables which the Professor so easily jots down. "Bochart says that *Chimia* was first mentioned by Firmicus." "The word *Chimia* is not of legitimate Arabic formation. The Arabian lexicographers are unable to arrange it under a verbal root; they merely explain what *Kimia* can do, not how the word originated. Therefore Freitag, in the fourth volume of his large Lexicon, explains the word, but calls it a foreign importation. Bochart's notion to derive it from *Kama,* 'to conceal,' because it was an occult science, is no revelation. No adept gives such a title to his own profession."

"The Arabians took the word as the Romans and Greeks had done before, where they found it, viz. in Egypt, the Greek inhabitants of which, at the time of the Mohammedan conquest, called that science by a name closely resembling that by which it was known in Europe, although it was of old Egyptian, Hamite origin. The Arabians made its acquaintance in the form of *χημεια* or *χημια.* This sound they transcribed in Semitic characters, putting the כ for the Greek *χ*. That this was an orthodox practice, the Talmud incidentally but fully proves. In the treatise

'Kerithuth' (or excision) of the Babylonian Talmuds, folio 5 *b*, the following occurs:—'The anointing of a king is in the form of a diadem, the anointing of a priest in the form of a Greek ki (כי).' Now a Greek ki is χ, consequently Semitic K was considered the equivalent of Greek χ, and *χημεία* or *χημία* ought to be writtten as it is written, כימיא, kimia." "Egyptian etymology, then, ought to be consulted. It is not probable, although it is possible, that the Egyptians named it from their country; for natives do not think of naming a produce of their own, whether abstract or concrete, after their own country; foreigners do that for them. Then the Egyptian term must have been chosen for the connexion between the science of χημία and the idea of *heat*" (blackness); "but," he adds, "I have not succeeded in discovering an express declaration to the effect that chemistry is called so because it deals with fire or heat."

Thus far Professor Theodores has confirmed my idea, and with learning that I could not muster.

He also adds:—"As in Semitic, so in Egyptian, 'black' is a derived meaning;" and I am glad to have this authority for altogether throwing aside the word "black" as connected with chemistry.

Prof. Theodores came to the conclusion that, although the word was found in Semitic, it was of Hamitic origin; and he agreed with me that it was needful to go a step onwards or to the side, and adds, "Bunsen, in his large work 'Die Stellung Aegyptens in der Weltgeschichte,' vol. v. sect. 2, in the Etymological Table, inserts:—'*Hem*, or *'hem*, sieden, glühen, = Hebrew *ham*. As *'h* is related to *s*, the Egyptian *Sôm* has the same value, and with this the Teutonic *Sommer*. *Sommer* has some connexion through *Sun, Sonne*.' This is in Bunsen."

By this reasoning we are fairly removed from the Arabic,

and partly from the Hebrew, and are in Egypt. However, it did not seem to me well to stop there. We cannot tell the true connexion of Egypt with Assyria and with the old Accadians; and we know that magic was highly cultivated in the East by the latter people. It was necessary, therefore, to seek in that direction; and I wrote to Dr. Birch, as it seemed to me impossible to obtain this information direct without some knowledge of the language; but perhaps the late dictionaries and other aids might have served me so far.

Dr. Birch's letter is very explicit so far as Assyrian and Egyptian are concerned. He says:—"The word for 'heat' in Assyrian is *Hhamamu* or *Khamamu*. In Egyptian we have *Khemt*, 'fire,' 'to warm.'" Then he gives "*Qam*, 'Egypt,' or 'the black land,' *qam* meaning in Egyptian 'black.'"

I then went to Prof. Sayce's 'Vocabulary of Assyrian and Accadian,' under the guidance of Mr. Harry G. Rylands, of the Biblical Archæological Society; but we got nothing out of the Accadian: the words out of the Assyrian, however, are interesting:—

Samsu, the sun.
Samu, the heavens.
Samas, the sun-god.
Khamamu, heat.
Khammu.
Khamsa, fire.
Khamdu, light.

Here, then, seems the true origin of the word. The meaning is intelligible, worthy of the people and worthy of the science. The question, however, again occurs, Was it Egyptian or Assyrian? The reasons for believing the primitive not to have originated in Egypt may stand; but

we must learn, I suppose, whether a Semitic people took the word to Egypt and gave it that Hamitic cast which Prof. Theodores remarks. It seems, however, to be allowed by later writers that some at least of the Egyptians came at an early period from Asia, although they seem to have been much mixed during their time of power.

The connexion of the Egyptian with the Semitic languages has been long believed; and Brugsch-Bey is very strong in his belief against its African origin. He says (vol. i. p. 3):—"The Egyptian language, which has been preserved on the monuments of the oldest time, as well as in the late Christian manuscripts of the Copts (the successors of the Pharoahs), shows in no way any trace of a derivation and descent fron the African families of speech. On the contrary, the primitive roots and the essential elements of the Egyptian grammar point to such an intimate connexion with the Indo-Germanic and Semitic languages, that it is almost impossible to mistake the close relations which formerly prevailed between the Egyptians and the races called Indo-Germanic and Semitic."

In Upper Egypt, as we find from the same author, there was a city called Tini, now entirely out of existence, but once giving the title of Princes of Tini to the highest functionaries of the blood royal. From this place the first of all Egyptian kings was descended, namely Mena or Menes. In the time of the Romans it was known only from its dyers in purple. This town is mentioned here because of its arts; but it serves my argument chiefly because we find almost opposite to it, in the district of Chem, the town of Chemmis mentioned in Herodotus. This town had as chief deity Chem or Khim, who was the representative of the heat of the sun and the transmuter of death into life as we may say, as well as the producer of the fruits of the earth. This name was translated by the

Greeks into *Pan* from some supposed similarity of characteristics. This Greek and Roman habit of translating or giving equivalents for names has caused a great deal of confusion.

We have seen that Zosimus was one of the earliest chemists; and he is called Panopolites, a citizen of Panopolis. If the Greeks had not translated the name, he would have been called Zosimus of Chemmis, or Zosimus the Chemmite (the chemist); and then probably no one would have disputed the meaning. And here he stands before us with his description of metals and of distillation, and with drawings of his alembics, and what appears as a crucible with fire under it.

I have said that I believed the origin of the word to be "heat;" but it is not necessary that it should have taken the leap at once. It is probable that the name came directly from the town of Chemmis or the district of Chem, whilst the name of the town came from the god Khem, the representative of solar heat. The common people would think of the town, the more scientific of the character or quality.

Herodotus tells us of two places in Egypt called Chemmis; the second is a floating island in Lower Egypt, at Buto. It is one of the wonders that people here would not believe until a similar one was found in England, although there were several on the Thrasimenian lake not far north of Rome, and also on the Tarquinian.

The mysterious word *Imouth,* which is connected with chemistry, Pan, and Æsculapius, has been said to be an Egyptian word for "chemistry;" but if we look at *Khem,* we have a clue to *Imouth* also. *Khem,* in the widest sense as the producer, "was the father of his own father;" and *Mauth* was the abstract idea of mother, who consequently proceeded from herself (this from Wilkinson); or, the

goddess with the sun's disk had a son Imhotep (in Greek *Imuthes*), the Æsculapius of the Egyptian mythology: this again connects with the sun and with medicine, a branch of chemistry. Here nothing appears to me to be forced, but all comes in naturally. All the chemical allusions to "Imuth" may be seen in Kopp's work (Beiträge, &c.) alluded to. I do not here attempt to collect the remains of chemical history at any point, but to lead up to the view which I wish to illustrate.

We see, then, that the word *Khem* has no trifling meaning, but relates to the principle in nature which, even now, in the latest of times, we look on as the great promotor of all life both on the earth and beyond it. It goes directly to the sun and coincides with our own ideas on the subject. It goes also to mental emotion and to violence, as we saw in the rage of the serpent. It includes internal action, but excludes the glory of the sun, which is represented by the sun-god Ra, translated by the Greeks into Apollo. Still the qualities are mixed. The floating island Chemmis had a temple on it dedicated to Apollo, according to Herodotus. Had he mistaken the heat-god of the chemists and the farmers for the god of light, the glorious Apollo? The difficulty of separating the two is probably the real cause of the mixture of qualities in Chem.

Thus far it seems natural that "chemistry" should come from *Khemi*, and from the town Chemmis in Khemi, the spot where the god Khem ruled, and that the earliest of chemical writers, or, at least, one of them, and the best-known of the earliest, should have come out of Chemmis, and the name of the art or science (as it may, to some extent, be considered) should have been preserved as "chemia." It seems so far to have been connected with metals, distillation, dyeing, and medicine even in the earliest ages.

We may go a little further, namely into the hills east of Coptos, and see where chemistry was practised by the workmen. Probably Coptos was another place devoted to Chem, or, rather, in a district of Chem. Chem was the "land of Coptos;" and we learn that there was a road from Coptos to the Red Sea among the mountains where the valley of Hammamat is, and where the Egyptians obtained stones for building as well as gold- and silver-ore. I suppose the word "Hammamat" to have also to do with heat (I am told that it sounds as if an Arabic inflexion); and if the ores were treated near or at the mines, there would be no wonder. The heat of the district would perhaps suffice to give the name—although there are many hot places east and west of Egypt, and the probability rather is that the Arabic form has merely grown out of the old Egyptian, the root being the same.

The god Chem is called "the master of the tribes which inhabit this valley," and "the lord protector of the mountain;" and Brugsch-Bey also tells us that the traveller said his prayers to him, and cut out "in tablet and holy characters his reverence for the god."

We learn in the rock-inscription of Hammamat that the Arabian desert and the coast adjoining it was called the "land of the gods;" and to this the valley led.

As an incident in connexion with this, it is interesting to read of a journey made by king Seti to see the mines in the fourteenth century B.C. :—"After he had mounted up many miles, he made a halt to take counsel with himself and to come to a conclusion upon the information he had received, that the want of water made the road almost impassable, and that travellers by it died of thirst in the hot season of the year."

From the monuments we learn, still following Brugsch-Bey:—"He had the well bored for them. Such a thing

was never done before by any except him the king. Ye gods of the well, assure to him your length of life, since he has made for us the road to travel upon, and has opened what lay shut there before us, and the way has become good. Now the gold can be carried up, as the king and lord has seen. All the living generation and those which shall be hereafter will pray for an eternal remembrance of him," &c.

In another place a great deal of water is spoken of as being found. The well mentioned, although 120 cubits deep, had dried up; but Ramses the second again bored other wells at the instigation of the Prince of Cush, "who said that the land was accursed for want of water." At this time water appeared at 12 cubits depth; brooks were formed; and fishermen from the islands came, enjoyed themselves, and sailed on the water. Whether this water was used for washing the ores, or not, I do not see mentioned*.

It is difficult to stop adding near relationships to things chemical in the land of Egypt; but one I must add. It certainly seems strange that the Egyptian name for Hermopolis should be Khimunu, as if also related to *Khem*; and it brings to mind the name of Hermes, the *thrice great* and of Egyptian fame, among the alchemists an authority for final appeal.

We have passed to Egypt and touched on Assyria, finding there the same word. There may be reason to believe that this word is originally from Asia; and it may have come from its centre: at any rate it has connexions there. We find the word *Kem* there with the same chief meaning, "heat." I was very much inclined to think that Aryans or Semites coming from the north called Egypt hot; but the idea of blackness has possessed so many

* Brugsch, vol. ii. p. 83.

people that I suppose it is not to be neglected. If the land was not black, I was inclined to say that the people were black and had gradually been driven out. But it may be too far to go back. The land must have differed in early times. There cannot be metallurgy without heat; and for that we must have fuel. Have Egypt and Midian made a desert of themselves in part and of much of their surroundings by burning up their forests? Civilization of a high character without fuel is impossible in any country known to us. Want of wood has made much of the ancient world desolate, and keeps it so now; and the world has much to thank the Germans for in teaching the cultivation of forests. Without this care Germany would have had no position as a great power.

In Egypt wood, and therefore fires, must have been a scarce thing for ages; people have lived on uncooked food; civilization has been essentially stagnant for want of fuel. There is no wonder that an idea arose that heat could do any thing. Did it not warm the earth which even the Nile could not make fertile alone? and did it not bring gold and silver, iron and brass out of stones? It turned rosin into strange oils; and it brought strange liquids, essences, and medicines with wonderful virtue out of vegetation. Wonderful drinks were prepared; and are we to be astonished if one of them was also connected with Kemi, that appeared to renew man's life and make him stronger and wiser? We have seen that *Hama* became (in an apparently wonderful philological manner, however simple when explained) a relative to *Soma* and *Summer* and the *Sun* which warms up all nature; and it is fair to go to Old Indian words to seek for new analogies. There is the liquid Soma, which is "pressed out of the *Asclepias acida*, making an intoxicating drink, used by the gods to strengthen them in their fight with demons" (see 'Indo-

germanische Chrestomathie,' written by H. Ebel, A. Leskien, Johannes Schmidt, and August Schleicher: Weimar, 1869).

In Colebrooke's Essays we find that "sacrifices with fire" are connected with "the drinking of the milky juice of the moon-plant or acid Asclepias*, and furnish abundant occasion for numerous prayers adapted to the many stages of these religious rites."

This milkiness does not favour the idea of distillation; but as there are treatises on the preparation of the juice, I dare say the usual plan may be known. A good deal has been written on the plant, I believe. Indra and other subordinate deities are made to say, "We have drunk of the juice of the Soma and are become immortal, we have attained effulgence, we have learnt divine truths. How can a foe harm us? How can age affect the immortality of a deathless being?"

If, however, we inquire what is the quality of this drink Soma, we get into some difficulties. At least, chemical books give us most unsatisfactory information. We are there informed that the juice is milky and contains a crystalline substance called asclepiadine and asclepion, bitter, purgative, and emetic. Now such a drink is not that which the gods would be supposed to drink to strengthen themselves against demons.

From one of the Asclepiads (*Pseudosarsa* or *Hemidesmus indicus*) a substance was obtained called Indian sarsaparilla. Dr. Ashburner introduced it from India as improving the general health, "plumpness and clearness and strength succeeding to emaciation, muddiness, and debility." The Asclepiads are also said, in Pereira's 'Materia Medica,' to possess stimulating properties. I cannot give more of the properties of this plant; and I think that the mode in which

* Soma-lata, *Asclepias acida* or *Cynanchum viminale*.

chemists have treated it in analysis has prevented its fermentation; or, rather, they have extracted one peculiar principle and left the rest unknown. From other works I learn that there are various characters to the juices of the various Asclepiads.

In speaking of Old Indian or Sanskrit, I am quite aware that it is the chief of a family of languages different from the Semitic; but I have already shown what Brugsch has said of their relations; and we could confidently reason from the ready way in which some inventions pass from nation to nation, keeping their original names.

But we must attend a little more to this drink Soma. We have seen that *Kemi* goes readily into *Chem* and into *Hem* or *Ham*, and that *S* becomes a substitute for *H*; and this must be borne in mind.

In the Sanskrit we have the words *Soma* and *Kāmū* curiously connected—the first meaning the celebrated drink that strengthens the hearts of the secondary gods as well as of men and makes them immortal, whilst the second, *Kāmū*, is the vessel into which it flows*. Whether this means flowing from the plant or not I do not learn.

Again, we find it mentioned, by the authors quoted, as found in old Bactrian in *Haoma*, the name of a plant from which a sacred drink was prepared, also of the genius of the same, as if it alluded to Chem also. To this, however, is added that it comes from the root *Hu*, "to press out," "to prepare." This I bring in, not as strengthening the argument, but as necessary to remember; for, by pushing the inquiry further, it may be found that the preparing was a mode of warming up; but this is more likely to be a mere fancy.

In the East we also find Kama-deva as Cupid; and Mr. William Simpson sends me the following from his stores:—

* Schleicher's 'Indogermanische Chrestomathie.'

"Muir, in 'Sanscrit Texts,' vol. v. p. 402, refers to the 'Rig-Veda,' x. 129, 4, where *Kama* is identified with the idea of Ἔρος as the first of all the gods, according to Hesiod. Dr. Muir also refers to the 'Atharva-Veda,' iii. 21, 4, where he says *kama* is distinctly identified with *agni*, the Sanscrit for 'fire.' In the Asiatic researches, in an article by Wilford, in which he identifies the *cama* or *kama* of India with the *chemia* or *chemi* of Egypt, he says, 'It has been conjectured that the more modern Greeks formed the word *chemia* from this name of Egypt, whence they derived their first knowledge of Chemistry.' Gesenius points out the similarity of the Sanscrit *kam* with the Hebrew."

Thus *Chem* is connected with heat from Africa to India, and in a secondary way with an "elixir of life," whilst gradually it has been made to mean that science which does so many wonders by means of heat, having reference both to the effects in external nature and the analogous influence in the temper of man and the lower animals.

We have followed the words far, and everywhere have come upon "heat;" and we find that there were many chemical operations used in Egypt which required heat, but that the word did not consolidate itself so as to mean a science in Egypt; at least it did not appear to have done so to the earliest writer who is known to have used the word with somewhat of our meaning. Firmicus was not a man to understand a science; besides he was an astrologer, and the age in which he wrote was one in which the world was getting into confusion; he could only hear whispers of truths in nature. The discussion on his position in Kopp's book is interesting. We see also, under the names Zosimus and Democritus, that all of these men had a limited, merely practical and unscientific view of things, behind even what we may suppose to have existed under the very

orderly method in which knowledge was arranged in Egypt. As to the secrets, we know well that the Egyptians had power enough to keep them; and even when they tried to convey knowledge, we, after a couple of thousand years, find it a very difficult thing to read any of their writing, and he who can read a few lines is looked on by us as a very learned man.

It seems, then, clear that chemistry came out of the darkness with its name connected with metals in the fourth century; and in the works of Zosimus we find distillation treated of side by side with metals, he and Synesius and Democritus looking to Egypt as the land where all was to be learnt. This I need not prove, but refer to Kopp's 'History of Chemistry,' and to Hoefer, and to Olaus Borrichius, whose writings all must consult. In Zosimus we have metals, "furnaces, and apparatus" spoken of; and he came from Panopolis; he is sometimes called also of Alexandria. Until the contrary be proved wise, we seem obliged to start clear from him as from a landmark of the ending of the period of Egyptian secrecy, although we learn too little from him.

These facts enable us to trace the idea of chemistry as it gradually consolidated itself:—going back in time to early Egypt, where the word was used for "heat" in various relations; going into Syria and Palestine, where the word was also used for "heat," and for "rage" or mental heat, and for violence caused by intoxication; moving onwards to the Assyrians, who also had the word as meaning heat; and following it into Bactria and India, where it associated itself with intoxicating liquors as well as with heat, and where we find the vessels called *kamu*, as if in imitation of the vessels for heat or the crucibles which must have existed in Egypt.

A chief intoxicating liquor of Asia still retains the name;

and *koumiss* comes from the horse-feeding plains to Russia and the West. I do not know the history of the word; but the likeness is remarkable, and the character likewise.

People may say that I have imagined something; but it is easy to find out what is imagined and what is not. The statements or facts are not deniable; one can only deny the connexions which have been made.

I am going, however, to add a little which may be less certain to some minds. Certain people are called very *reliable*—those who never reason, but merely collect facts: they are safe, but never go far; we could not learn much if we had only our touch instead of eyesight. Let us speculate a little. When we look for "the elixir of life" among early alchemists, we find the introduction of silver and mercury into its preparation; and I will not inquire who was the earliest who did this; but if we go far enough back in point of space and time, we find the "elixir of life" to have been an intoxicating liquid. The long time of preparation, the wandering search after a proper heat (which in some cases, at least, was quite that of fermentation), and the confused ideas of distillation put into bombastic words among the alchemists lead me to suppose that they had traditions of the preparation which were lost as to clearness, whilst they point towards the formation and distillation of alcohol. In the far East the precious liquid is simply an intoxicating one; the peculiarity of the *Asclepias* found by chemists exists to a small extent only, and did not show itself with sufficient prominence to be noticed in the East.

This confused idea of early men, who, when they were excited by drunkenness, fancied themselves elevated above common life, and imagined that if they could obtain enough of the liquid the elevation would last for ever, passed readily into the notion of an elixir; and we see that the gods themselves used it. This idea kept its place even in

Palestine; and we have in the Bible itself (Judges, chap. ix. verse 13):—"The vine said, Should I leave my wine, which cheereth God and man?" evidently a quotation not unfamiliar to the people of the time.

Whilst, then, it is certain that an intoxicating liquid was an *elixir vitæ* of the East, and *hama* was a word used for the heat of intoxication among a Semitic people, we move onwards to Zosimus teaching chemistry and distillation, and uniting the operations of metallic chemistry and vegetable. The Arabs, naturally more eastern, with other Asiatics, gave more study to the elixir, as if holding the tradition of the Soma. However, there is an old drink mentioned among the Egyptians which partook of the qualities of the elixir, and in the preparation of which they used honey, a ready former of alcohol.

And after all, some may say, Was the Elixir of Life, after which so many earnest men have striven, nothing but alcohol, mere brandy and whiskey? I fear it really was, and that the memory of the past was a delusion. This idea has been held by others. We have the same idea still; and the name is retained in the words *aqua vitæ*, which mean simply the elixir or water of life. The elixir is, according to some, a "pure water." We have the delusion ready formed, and greedily preserve it among our population. Men still believe brandy to be life-giving, whereas many others know that for every benefit it confers it gives seven curses. Still it is made by heat, it produces heat, it is *kemi*.

There have been many such delusions: respecting opium there is a common one; tea itself has been said to cure all diseases; and that concerning alcohol is one of our oldest and most persistent.

The idea that gold was made by a liquid seems to have arisen from the properties of mercury. Throw it among the sand, stir it about, then heat it strongly, and you will

obtain gold if there was gold in the sand; and thus mercury came to be associated with gold. It works wonders; but this, also, is done by means of kemi (heat).

Take simple honey and add it to some plants, let it stand, and then apply heat. "The tree of the philosophers is extracted or drawn off in three; but the wine thereof is not perfected till at length thirty be completed."

"This is not water in its form, but fire, containing in a strong and pure vessel the ascending waters, lest the spirits should fly away from the bodies; for by this means they are made tinging and permanent or fixed." "Look into the sweetness of sugar, which is one kind of sweet juice, and into the sweetness of honey, which is yet more intense and inward. Except you make the bodies spiritual and impalpable, you know not how to purify ixir or to proceed in the work." These isolated sentences from the so-called Hermes point to fire and distillation; and I might end with a saying quoted from Van Helmont * :—"Let him who would learn buy coals and fire."

Addendum I.

It appears, then, that I go on a very different track from that followed by Prof. Gildemeister and Prof. Schorlemmer. I leave the Arabs as too late to have an opinion; and I can easily imagine how a Pacha's mind could mix all the uncertain ideas together and form some notion of a plant called kimia which changed base metals into gold. I consider the connexion with "moisture" (χυμὸς) far-fetched, and had never any confidence in it. If, however, χυμὸς were considered rather in connexion with "pouring," it would have more probability; but then the original is χέω, "I pour," and the υ is found only in compounds.

* I have not the original beside me.

Addendum II.

There is another derivation of the word "chemistry," mentioned by Mr. James Mactear, F.C.S. I have only received a few lines on the subject, an abridgment of a paper read at the Philosophical Society of Glasgow. In this it is said that Mr. Mactear traces the history of chemistry through the medical science of Greece, Persia, &c., and the word itself to *kham,* an Arabic word of Sanskrit origin meaning "five," or rather its compound *khamis* meaning "the fifth,"—adding, "As is well known, the ancient Hindoos recognized five elements—water, fire, earth, air, and ether, the latter being the principle or type of active force or motion which caused the changes in the condition of the elementary types or their combinations. From a consideration of these and other facts, he derived the title of the science from *al khamis,* 'the fifth,' meaning the science of force or change. No more perfect descriptive title could be found for it in our present enlightened age."

The scientific division of the elements is found in the atomical philosophy of Canáde, where ether comes in as the fifth existence, "infinite, one, and eternal." In the Sánkhya philosophy the first is a diffused etherial fluid occupying space. The further account of it and its power of influencing the other elements will, I dare say, be published by Mr. Mactear. I can refer at present those who are interested in the question to the 'Essays on the Religion and Philosophy of the Hindus,' by the late H. T. Colebrooke, a standard authority.

It cannot be agreed to give the Arabs more than the form of the word, or the *al* in *alchemy*; they are therefore left out,—unless the word "fifth" can be taken further back and shown in any Sanskrit dialect to have a connexion

with philosophy except as a mere enumeration. Indeed, we see that in the Sankhya philosophy ether comes first; but of more importance is the fact that we have no proof of the chemical arts being well known in India; and there is no historic indication of their existence connected with a system of philosophy. This is very conclusive, because Indian philosophy is well preserved: it is full of speculation and subtle abstraction; but it is not experimental; and from experiment must have arisen the great power which is in the hand of the chemist, the science of chemia.

The connexion between the "elixir of life" and *aqua vitæ* is by no means new, although the mode of approaching it here may be so.

XV. *Notes on a Bore through Triassic and Permian Strata, lately made at Openshaw.* By E. W. Binney, V.P., F.R.S., &c.

Read February 10th, 1880.

For many years attention has been given by local geologists to the district lying between the Manchester coal-field and that of Aston-under-Lyne and Oldham. The first authors that treated on it were probably Messrs. Conybeare and Phillips, in their 'Outlines,' published in 1822. Mr. James Heywood, F.R.S., in a paper published in vol. vi. (2nd series) of the Society's 'Memoirs,' also noticed it. In a communication of my own, published in the first volume of the 'Transactions of the Manchester Geological Society,' a horizontal section is given of the

country between Manchester and Waterhouses, showing one great fault as then known. Afterwards, in vol. xii. (2nd series) of our 'Memoirs,' evidence is given of another fault at Medlock Vale, and lately, in part vi. vol. xv. of the 'Transactions of the Manchester Geological Society,' Messrs. Bradbury and Atherton have shown a third fault at Openshaw. As the district is thickly covered by drift deposits, and few natural sections are exposed, we have to wait till evidence is afforded by borings and sinkings. In several papers by me, printed in the 'Memoirs,' information has been given as it was met with; and as Mr. John Bradbury's boring is one of the most valuable, I wish to add it to my other communications on the subject, in order to make them more complete.

Mr. Bradbury's labours have shown the Permian sandstone, the one so well exposed at Vauxhall, in our city, to be of greater thickness than hitherto proved in the district; and as this deposit is a most formidable difficulty in sinking to the underlying coal seams, it is desirable that all information respecting it should be given to the public.

The bore was made near to the Ashton canal in Openshaw, and adjoining the boundary of that township with Clayton. According to Messrs. Bradbury and Atherton it was as follows :—

	feet.
Drift deposits	36
Trias (Pebble-beds)	46
Permian marls, containing beds of limestone, one of which was 1 ft. 4 in. in thickness, and nodules of gypsum. In the lower parts of the marls and limestones were shells of *Schizodus obscurus*, *Gervillia antiqua*, &c.	200
Conglomerate	3
Permian sandstone	752
Upper Coal Measures, containg 12 beds of Ardwick Limestone, one of which is 5 ft. in thickness	263
	1300

The dip of the Permian strata was about 1 in 8, and that of the Upper Coal Measures 1 in 3, to the S.S.W.

The strata found resemble those at Medlock Vale, except that the Permian sandstone has increased from 420 to 752 feet in thickness. I have estimated that rock under Manchester at 400 feet; but its entire thickness has never to my knowledge been proved. In Chester Street, Chorlton-upon-Medlock, at the sugar-works of Messrs. Fryer & Co., on the south side of the fault which runs from N.W. to S.E. between that place and the late Mr. Green's dye-works in Brook Street, the Permian marls and conglomerate bed, increased to 260 feet in thickness, were found resting on upper Coal Measures containing the *Spirorbis*-limestone similar to that at Ardwick, without any trace of the Permian sandstone. Similar results have been found at the borings and sinking of the Seedley print-works and the Patricroft colliery; and very lately Dr. Perrin informed me that the same occurred in a sinking at Plank-lane Colliery, near Leigh.

All the facts hitherto observed appear to show that the Permian sandstone is found of great thickness under the district lying between the Manchester coalfield and that of Ashton-under-Lyne and Oldham, while to the south of Manchester, under the Trias, it is replaced by the conglomerate of increased thickness. The former rock has probably never been deposited; nevertheless the fact of its general absence is of great importance to all parties who may sink for coal under the Trias.

At the present time the Permian strata of the N.W. of England and the S.W. of Scotland, so far as my knowledge extends, are represented in Lancashire in the following descending order:—

1. Upper Permian sandstone of Moat, Shawk, St. Bees, and Furness Abbey; absent in South Lancashire, unless

there is a representative of it in the Knowsley Quarry, near Prescot.

2. Magnesian marls with limestones and gypsum, containing *Schizodus obscurus*, *Gervillia antiqua*, and other characteristic fossils.

3. Conglomerate.

4. Permian sandstone of Vauxhall, Manchester.

5. Lower Permian sandstone of Whitehaven and Astley, by many English geologists taken to be unconformable Coal Measures, but in Germany termed Lower Rothliegende.

The old Magnesian Limestone formation, as described by Professors Sedgwick and King, and my friend Mr. J. W. Kirby, in the East and N.E. of England under the four first-named divisions, was pretty plain, although the line of demarcation between the Brotherton limestone and the Trias was not so easy to make out in all places. In the N.W. of England, and adjoining Scotland, the St.-Bees sandstone, a rock of about 1000 feet thickness, cannot be traced passing distinctly upwards into the Trias, although doubtless it does somewhere betwixt Carlisle and the Solway; but in the valley of the Irk at Manchester the beds Nos. 4, 3, and 2 are seen lying on each other, apparently passing into the overlying Trias, all the three rocks dipping at the same angle and in the same direction.

Near Manchester the occurrence of Permian fossils has enabled us to fix the position of the red sandstones and marls of the Trias and Permian beds; but after leaving Barrow Mouth, near Whitehaven, and traversing the country by Maryport, Carlisle, and Longtown to Canobie, as yet no fossil organic remains have been met with to help us, and we have to trust chiefly to superposition to separate the two formations. All who have investigated these formations know the difficulty of

determining a Permian from a Triassic sandstone by external characters.

In some places in Lancashire the Coal Measures are covered by Triassic beds without the occurrence of the intermediate Permian beds; but near Manchester the latter are generally met with either as the upper deposits, the marls and the conglomerate (Nos. 2 and 3) most frequently together, or with all the three beds of the series.

XVI. *On an Adaptation of the Lagrangian Form of the Equations of Fluid-Motion.*—Part I. By R. F. Gwyther, M.A.

Read April 20th, 1880.

I.

The object of the Lagrangian form of equation is to follow the motion of a particular element; and although the Eulerian forms suit the general purposes of fluid-motion best, there are certain cases, as that of vortex motion in a perfect fluid (which may be termed steadily progressive), where the course of an element may be investigated with advantage.

For this purpose I propose investigating the course of a fluid element, defined by means of surfaces moving with the fluid, and expressing the results as far as possible in terms of the parts of the element.

This method leads to a more general integral form than that of Weber, and finally exhibits some of the known properties of fluid-motion in a novel manner*.

* A similar method with the ordinary coordinates has previously (Q. J. of Math. Feb. 1880) been used by Mr. Hill to obtain some similar results, which will be referred to later.

If ϕ be a scalar function of the position of a point in the fluid, its total differential after time δt is $D_t\phi\delta t$; and if $D_t\phi=0$, the property of the point of which ϕ is the analytical expression is unchanged during the motion. Now let $\phi=\mu$ be the equation to a surface all points on which enjoy the same property; such a surface will, if $D_t\phi=0$, move with the fluid.

The number of independent surfaces of this kind which can pass through any point, or the number of independent integrals of the equation $D_t\phi=0$, is three, or the dimensions of the space considered; for the Jacobian of a higher number would vanish.

The position of a point in the fluid in motion may be represented in terms of the three independent sets of surfaces $\phi_1=\mu_1$, $\phi_2=\mu_2$, $\phi_3=\mu_3$, where μ_1, μ_2, and μ_3 are parameters.

I have avoided the use of theorems and terms such as those of "circulation," as I think they are apt sometimes to override and hide the more important facts which their discoverers intended them to express; and I have endeavoured to bring the fundamental properties to the surface.

Imagine an element of the fluid separated from the rest of the fluid by the surfaces which we shall denote by

$$\mu_1,\ \mu_2,\ \mu_3,\ \mu_1+\delta\mu_1,\ \mu_2+\delta\mu_2, \text{ and } \mu_3+\delta\mu_3.$$

We will investigate expressions for the parts of this element.

First, $\nabla\phi_1$, $\nabla\phi_2$, $\nabla\phi_3$ represent vectors in the directions of the normals to the three surfaces, such that if h_1, h_2, h_3 stand for their tensors, the thicknesses of the element will be given by $\frac{\delta\mu_1}{h_1}$, $\frac{\delta\mu_2}{h_2}$, and $\frac{\delta\mu_3}{h_3}$ respectively.

The directions of the edges are given by $V\nabla\phi_2\nabla\phi_3=a$,

say, and the similar quantities β and γ, so that

$$V\beta\gamma = -S\nabla\phi_1\nabla\phi_2\nabla\phi_3 \cdot \nabla\phi_1 = H\nabla\phi_1, \text{ say.}$$

The length of an edge will be given by the thickness divided by the cosine of the angle between the corresponding normal and the edge. Thus length of the edge a is

$$\frac{\delta\mu_1}{h_1}\frac{h_1 Ta}{S\nabla\phi_1 a} = -\frac{\delta\mu_1 Ta}{H},$$

and the vector edge is

$$-\frac{\delta\mu_1}{H}a; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

whence the area of the face ϕ_1 is

$$\frac{\delta\mu_2\delta\mu_3}{H^2}TV\beta\gamma = \frac{h_1}{H}\delta\mu_2\delta\mu_3,$$

and the vector of the face

$$\frac{\delta\mu_2\delta\mu_3}{H}\nabla\phi_1. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

From either (1) or (2) we may get volume of element

$$= \frac{\delta\mu_1\delta\mu_2\delta\mu_3}{H^3}S\alpha\beta\gamma = -\frac{\delta\mu_1\delta\mu_2\delta\mu_3}{H}. \quad . \quad . \quad . \quad (3)$$

The condition of continuity is therefore that $D_t(dH^{-1}) = 0$, where d denotes the density; and this condition in an incompressible fluid becomes $D_t H = 0$. Generally $d = H\phi$ where $D_t\phi = 0$.

Before proceeding to investigate any forms for σ, the velocity, we will find some expressions from the action of D_t on the expressions just found.

Remembering that

$$D_t = \frac{d}{dt} - S\sigma\nabla,$$

we see that

$$D_t\nabla = \nabla D_t + \Delta(S\sigma\nabla)$$

symbolically, where Δ is the same operator as ∇, but acts on the σ only.

Thus

$$D_t\nabla\phi = \nabla D_t\phi + \Delta(S\sigma\nabla\phi) = \nabla D_t\phi + \nabla\phi_1 S\frac{d\sigma}{d\phi_1}\nabla\phi$$
$$+\nabla\phi_2 S\frac{d\sigma}{d\phi_2}\nabla\phi + \nabla\phi_3 S\frac{d\sigma}{d\phi_3}\nabla\phi, \quad . \quad . \quad . \quad . \quad (4)$$

since

$$\nabla = \nabla\phi_1\frac{d}{d\phi_1} + \nabla\phi_2\frac{d}{d\phi_2} + \nabla\phi_3\frac{d}{d\phi_3}.$$

[These other two forms of ∇ will also be used,

$$-H\nabla = \nabla\phi_1 S\nabla\phi_2\nabla\phi_3\nabla + \nabla\phi_2 S\nabla\phi_3\nabla\phi_1\nabla + \nabla\phi_3 S\nabla\phi_1\nabla\phi_2\nabla$$

or

$$= \alpha S\nabla\phi_1\nabla + \beta S\nabla\phi_2\nabla + \gamma S\phi_3\nabla,$$

since ∇ is in form a vector.]

Let us first apply these results to $S\nabla\phi_1\nabla\phi_2\nabla\phi_3$ or $-H$; therefore

$$-D_tH = SD_t\nabla\phi_1 . \nabla\phi_2\nabla\phi_3 + S\nabla\phi_1 D_t\nabla\phi_2 . \nabla\phi_3 + S\nabla\phi_1\nabla\phi_2 D_t\nabla\phi_3$$
$$= -HS\left(\frac{d\sigma}{d\phi_1}\nabla\phi_1 + \frac{d\sigma}{d\phi_2}\nabla\phi_2 + \frac{d\sigma}{d\phi_3}\nabla\phi_3\right),$$

or

$$\frac{1}{H}D_tH = S\nabla\sigma = -HD_t\left(\frac{1}{H}\right), \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

or is evidently rate of compression per unit of volume.

If, again, we act similarly on α or $V\nabla\phi_2\nabla\phi_3$,

$$D_t\alpha = VD_t\nabla\phi_2\nabla\phi_3 + V\nabla\phi_2 D_t\nabla\phi_3$$

$$= S\left(\frac{d\sigma}{d\phi_1}\nabla\phi_2\right)V\nabla\phi_1\nabla\phi_3 + S\left(\frac{d\sigma}{d\phi_2}\nabla\phi_2\right)V\nabla\phi_2\nabla\phi_3$$

$$+ S\left(\frac{d\sigma}{d\phi_2}\nabla\phi_3\right)V\nabla\phi_2\nabla\phi_1 + S\left(\frac{d\sigma}{d\phi_3}\nabla\phi_3\right)V\nabla\phi_2\nabla\phi_3$$

$$= S\nabla\sigma.\alpha - S\frac{d\sigma}{d\phi_1}\nabla\phi_1.\alpha - S\frac{d\sigma}{d\phi_1}\nabla\phi_2.\beta - S\frac{d\sigma}{d\phi_1}\nabla\phi_3.\gamma$$

$$= S\nabla\sigma.\alpha + H\frac{d\sigma}{d\phi_1};$$

therefore

$$\frac{1}{H}D_t\alpha + D_t\frac{1}{H}\alpha = \frac{d\sigma}{d\phi_1},$$

or

$$D_t\frac{\alpha}{H} = \frac{d\sigma}{d\phi_1}, \quad \ldots\ldots\ldots \quad (6)$$

whence we obtain

$$\nabla\sigma = \nabla\phi_1\frac{d\sigma}{d\phi_1} + \nabla\phi_2\frac{d\sigma}{d\phi_2} + \nabla\phi_3\frac{d\sigma}{d\phi_3}$$

$$= \nabla\phi_1 D_t\frac{\alpha}{H} + \nabla\phi_2 D_t\frac{\beta}{H} + \nabla\phi_3 D_t\frac{\gamma}{H}. \quad . \quad (7)$$

Now

$$\nabla\phi_1\frac{\alpha}{H} + \nabla\phi_2\frac{\beta}{H} + \nabla\phi_3\frac{\gamma}{H} = \frac{S(\nabla\phi_1\alpha + \nabla\phi_2\beta + \nabla\phi_3\gamma)}{H}$$

$$+ \frac{V(\nabla\phi_1\alpha + \nabla\phi_2\beta + \nabla\phi_3\gamma)}{H},$$

of which the vector part is seen to vanish and the scalar becomes -3, whence

$$\nabla\phi_1 D_t\frac{\alpha}{H} + \nabla\phi_2 D_t\frac{\beta}{H} + \nabla\phi_3 D_t\frac{\gamma}{H} = -D_t\nabla\phi_1\frac{\alpha}{H} - D_t\nabla\phi_2\frac{\beta}{H}$$

$$-D_t\nabla\phi_3\frac{\gamma}{H}$$

and

$$\mathrm{HV}\nabla\sigma=\mathrm{V}(\alpha\mathrm{D}_t\nabla\phi_1+\beta\mathrm{D}_t\nabla\phi_2+\gamma\mathrm{D}_t\nabla\phi_3). \quad . \quad (8)$$

Of these (7) gives the more convenient form, from which we obtain

$$\begin{aligned}\mathrm{HV}\nabla\sigma&=\mathrm{V}\nabla\phi_1\mathrm{D}_t\mathrm{V}\nabla\phi_2\nabla\phi_3+\ \&c.\\&=\mathrm{V}\nabla\phi_1\mathrm{VD}_t\nabla\phi_2\nabla\phi_3+\mathrm{V}\nabla\phi_1\mathrm{V}\nabla\phi_2\mathrm{D}_t\nabla\phi_3+\ \&c.;\end{aligned}$$

and applying to each of these expressions the formula

$$\mathrm{V}\alpha\mathrm{V}\beta\gamma=\gamma\mathrm{S}\alpha\beta-\beta\mathrm{S}\alpha\gamma,$$

we obtain

$$\begin{aligned}&\nabla\phi_3\{\mathrm{S}\nabla\phi_1\mathrm{D}_t\nabla\phi_2-\mathrm{S}\nabla\phi_2\mathrm{D}_t\nabla\phi_1\}+\ \&c.\\&+\mathrm{D}_t\nabla\phi_3\{\mathrm{S}\nabla\phi_1\nabla\phi_2-\mathrm{S}\nabla\phi_1\nabla\phi_2\}+\ \&c.\\&=\nabla\phi_1\{\mathrm{S}\nabla\phi_2\mathrm{D}_t\nabla\phi_3-\mathrm{S}\nabla\phi_3\mathrm{D}_t\nabla\phi_2\}+\&c.+\&c.,\end{aligned}$$

whence

$$\mathrm{S}\nabla\phi_2\mathrm{D}_t\nabla\phi_3=\mathrm{S}\nabla\phi_3\mathrm{D}_t\nabla\phi_2,\ \&c.,$$

if the motion is irrotational. Also expressing

$$\mathrm{V}\nabla\sigma=l_1\alpha+l_2\beta+l_3\gamma,$$

we find, by operating with $\mathrm{S}\nabla\phi_1$ on each side,

$$\begin{aligned}-\mathrm{H}^2l_1&=\mathrm{S}\nabla\phi_1\nabla\phi_2\mathrm{D}_t\beta+\mathrm{S}\nabla\phi_1\nabla\phi_3\mathrm{D}_t\gamma\\&=\mathrm{S}(\gamma\mathrm{D}_t\beta-\beta\mathrm{D}_t\gamma);\end{aligned}$$

therefore

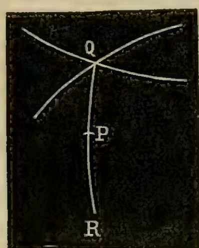
$$-l_1=\mathrm{S}\left(\frac{\gamma}{\mathrm{H}}\mathrm{D}_t\frac{\beta}{\mathrm{H}}-\frac{\beta}{\mathrm{H}}\mathrm{D}_t\frac{\gamma}{\mathrm{H}}\right),$$

and

$$\begin{aligned}-\mathrm{D}_tl_1&=\mathrm{S}\left\{\frac{\gamma}{\mathrm{H}}\mathrm{D}_t^2\frac{\beta}{\mathrm{H}}-\frac{\beta}{\mathrm{H}}\mathrm{D}_t^2\frac{\gamma}{\mathrm{H}}\right\}\\&=\mathrm{S}\left\{\frac{\gamma}{\mathrm{H}}\frac{d}{d\phi_2}\mathrm{D}_t\sigma-\frac{\beta}{\mathrm{H}}\frac{d}{d\phi_3}\mathrm{D}_t\sigma\right\}=\frac{\mathrm{S}(\mathrm{V}\nabla\phi_1\nabla)\mathrm{D}_t\sigma}{\mathrm{H}}.\end{aligned}$$

This form enables us to express in a general way the criterion that the vortex motion may exist in the form which it will be seen to take in a perfect fluid. Taking the two other similar quantities, and equating each to zero, we get $V\nabla D_t\sigma = 0$ or $D_t\sigma$ of the form ∇P; and, conversely, if $V\nabla D_t\sigma = 0$, then $D_t l = 0$

This notation also enables us to give a verbal expression for $V\nabla\sigma$. Thus: let P Q R lie on a line of intersection of two surfaces ϕ_2 and ϕ_3, and let them be distinguished by



ϕ_1, $\phi_1 + \frac{\delta\phi_1}{2}$, and $\phi_1 - \frac{\delta\phi_1}{2}$. Then the vector difference of the velocities at Q and R is $\frac{d\sigma}{d\phi_1}\delta\phi_1$; and resolving parallel to the face ϕ_1 of the element, having P for centre, these differential velocities on the parallel faces ϕ_1, we get

$$\delta\phi_1 V \frac{d\sigma}{d\phi_1} U\nabla\phi_1 \times \text{area} = \frac{\delta\phi_1}{h_1} \times \text{area } V \frac{d\sigma}{d\phi_1}\nabla\phi_1$$

$$= \text{volume} \times V\frac{d\sigma}{d\phi_1}\nabla\phi_1,$$

and similarly for the other faces. Therefore volume $\times\, V\nabla\sigma =$ resultant differential velocity and $V\nabla\sigma =$ mean resultant differential velocity per unit of volume, as the result of three shears and rotations. And by a similar method we may find the force due to viscosity, arising in consequence of the motion noticed above, upon

the faces ϕ_1, being due to the rate of change of the differential velocities just found. It will plainly be proportional to volume $\times$ $V\frac{d}{d\phi_1}(V\nabla\sigma)\nabla\phi_1$.

II.

The equation of motion is, on the generally adopted theory,

$$D_t\sigma+\nabla V+\frac{\mu}{3d}\nabla S\nabla\sigma+\frac{\mu}{d}\nabla^2\sigma=0, \quad . \quad . \quad (1)$$

where V represents what would be written, in Cartesian,

$$-\int(X\delta x+Y\delta y+Z\delta z)+\int\frac{1}{d}(\delta p);$$

and therefore assumes each of these quantities perfect differentials , and therefore ϱ a function of p only.

We will first take $\mu=0$.

In any case σ can evidently be written in terms of the normals to the three surfaces ϕ, determining the point at which σ is the velocity. Thus

$$\sigma=K_1\nabla\phi_1+K_2\nabla\phi_2+K_3\nabla\phi_3.$$

On the previous assumptions K_1, K_2, and K_3 must be of a definite form, which we proceed to find thus :—

$$D_t\sigma=D_tK_1\nabla\phi_1+K_1D_t\nabla\phi_1+\&c.$$

$$=\left(D_tK_1+K_1S\frac{d\sigma}{d\phi_1}\nabla\phi_1+K_2S\frac{d\sigma}{d\phi_1}\nabla\phi_2\right.$$

$$\left.+K_3S\frac{d\sigma}{d\phi_1}\nabla\phi_3\right)\nabla\phi_1+\&c.+\&c.$$

$$=\left(D_tK_1+S\sigma\frac{d\sigma}{d\phi_1}\right)\nabla\phi_1+\&c.+\&c.$$

$$=D_tK_1\nabla\phi_1+D_tK_2\nabla\phi_2+D_tK_3\nabla\phi_3+\frac{1}{2}\nabla(\sigma)^2, \quad . \quad (2)$$

whence the required form of the functions K_1, K_2, K_3 is given by

$$D_tK_1=\frac{dP'}{d\phi_1},\ D_tK_2=\frac{dP'}{d\phi_2},\ D_tK_3=\frac{dP'}{d\phi_3},$$

or

$$K_1=\frac{dP}{d\phi_1}+\Sigma_1,\ K_2=\frac{dP}{d\phi_2}+\Sigma_2,\ K_3=\frac{dP}{d\phi_3}+\Sigma_3{}^*,\quad (3)$$

where P is a scalar, and $D_t\Sigma=0$; $\therefore \Sigma$ is a scalar function of $\phi_1\,\phi_2\,\phi_3$ only, and σ takes the form

$$\nabla P+\Sigma_1\nabla\phi_1+\Sigma_2\nabla\phi_2+\Sigma_3\nabla\phi_3.\quad . \quad . \quad . \quad (4)$$

Proceeding to find $V\nabla\sigma$, from this we get

$$\begin{aligned}V\nabla\sigma&=V(\nabla\Sigma_1\nabla\phi_1+\nabla\Sigma_2\nabla\phi_2+\nabla\Sigma_3\nabla\phi_3)\\&=\left(\frac{d\Sigma_3}{d\phi_2}-\frac{d\Sigma_2}{d\phi_3}\right)\alpha+\text{\&c.}+\text{\&c.}\\&=\overline{\Sigma}_1\alpha+\overline{\Sigma}_2\beta+\overline{\Sigma}_3\gamma,\ \text{say,}\quad . \quad . \quad . \quad . \quad (5)\end{aligned}$$

where $D_t\overline{\Sigma}=0$, whence, if the motion is irrotational, the Σ's can be included in the ∇P under the above hypotheses.

Also $V\nabla\sigma$ may be written $H\overline{\Sigma}_1\frac{\alpha}{H}+H\overline{\Sigma}_2\frac{\beta}{H}+H\overline{\Sigma}_3\frac{\gamma}{H}$;

and since $-\frac{\delta\mu_1\alpha}{H}$ &c. denote the edges of the element, the components of $V\nabla\sigma$ are $-\frac{H\overline{\Sigma}_1}{\delta\mu_1}$ &c. times the corresponding edge; and if the fluid is incompressible this ratio remains unaltered in time; and the manner of variation, if the fluid is compressible, is indicated by the formula.

Also since $D_t\Sigma=0$, if $V\nabla\sigma$ is ever $=0$ it always was and will be $=0$, and the strength of the section of the vortex

* Conf. Mr. Hill's paper "On some Properties of the Equations of Hydrodynamics," Q. J. of Math. Feb. 1880.

on the surface ϕ_1 is $S\frac{\delta\mu_2\delta\mu_3}{H}\nabla\phi_1\overline{\Sigma}_1 a = \delta\mu_2\delta\mu_3\overline{\Sigma}_1$ and is constant in all time.

If we compare the form of σ here obtained with that assumed by Helmholtz, we find that his form is not sufficiently general, since he writes $\Sigma_1\nabla\phi_1 + \Sigma_2\nabla\phi_2 + \Sigma_3\nabla\phi_3 = \nabla\omega$, where ω is a vector with the condition $S\nabla\omega = 0$. That is, he includes two undetermined quantities instead of three, and does not express in a distinct form the essential condition (namely $D_t\Sigma = 0$) affecting them.

The quantities Σ are in no way dependent on finite forces which are acting (under the hypothesis), but entirely on the initial conditions (boundary conditions not now being entertained).

Unless the density be a function of the pressure only, the relations just proved will certainly not hold; for then

$$D_t\sigma + \nabla V + \frac{1}{d}\nabla p = 0,$$

and

$$D_t K_1 + \frac{d}{d\phi_1}\left(V + \frac{\sigma^2}{2}\right) + \frac{1}{d}\frac{dp}{d\phi_1} = 0.$$

$$\therefore K_1 = \frac{dP}{d\phi_1} + \Sigma_1,$$

where $\Sigma_1 = f(\phi_1,\ \phi_2,\ \phi_3$, and $t)$, in which case the fluid-pressures will of themselves cause vortex motion.

This seems to be a quite possible condition in a gas rapidly heated or expanded.

Now turning to the terms containing μ and imagining d a function of p only, and remembering that μ is independent of d, then

$$S\nabla\sigma = \frac{1}{H}D_t H = \frac{1}{d}D_t d = D_t \log d$$

will not necessarily be a function of d only, since $D_t d$ is not necessarily nor even generally so. Hence the conclusions we draw above will not generally hold good for viscous fluids, even in the cases where $\nabla^2\sigma=0$ or $\frac{1}{d}\nabla^2\sigma=\nabla Q$, where Q is a scalar. But if the fluid is incompressible, in either of these latter cases the theorems hold good.

Note.—Since writing the above I have seen a paper by M. Bresse, "Fonction des vitesses, extension des théorèmes de Lagrange au cas d'un fluide imparfait" ('Comptes Rendus,' March 8, 1880), in which he seeks to show that, if $\sigma=\nabla P$ at any time, it will remain so throughout the motion. In the investigation, however, the author follows Navier in taking $\frac{\mu}{d}$ as an absolute constant of the fluid. This, of course, will not lead to correct results for a gas, at any rate; and I think his result must be wrong for any liquid, even incompressible, as it would follow that vortex motion could not be generated, owing to the fluid-friction, if, for instance, it started from rest.

I should explain the defect in his reasoning thus:—Writing ϱ for $V\nabla\sigma$, and considering d constant, the equation affecting ϱ is

$$D_t\varrho+(S\varrho\nabla)\sigma+\frac{\mu}{d}\nabla^2\varrho=0, \text{ or } V\nabla D_t\sigma+\frac{\mu}{d}\nabla^2\varrho=0,$$

from which M. Bresse concludes that, if ϱ is at any time zero, it remains so. Now this seems to require that, for a small value of ϱ, $\nabla^2\varrho$ should not take a value of an infinitely greater order. It has been shown by Maxwell, that if ϱ be a vector function of any point, $-\frac{r^2}{10}\nabla^2\varrho$ represents the difference between the value of ϱ at that point and its

mean value over a small sphere of radius r about the point; and therefore I conceive that we cannot conclude from the above equation that vortex motion may not arise in lines or surfaces, but merely that it could not appear in a solid form, a form of the existence of which we have no evidence. The true criterion would be found by equating to zero the expressions found above.

Although I conceive that the theorem is not proven for the case which M. Bresse considers (where μ "may have any value other than zero)," I think that if μ is sufficiently small a proof may be given. For if μ were zero the proposition is true, and the terms owing to which it departs from the truth will appear with μ as a factor, and may therefore be omitted from the term $\mu\nabla^2\varrho$; and under this hypothesis, if $\mu\nabla^2\varrho$ is ever zero it will continue so, and the proposition is completed.

III.

Considering how the portion $\Sigma_1\nabla\phi_1+\Sigma_2\nabla\phi_2+\Sigma_3\nabla\phi_3$ of the velocity can be impulsively generated, we see that the initial equation of motion will take the form

$$\sigma=\int_0^T\psi dt+\int_0^T\frac{1}{d}\nabla p dt,$$

where T is the infinitely small time during which the impulsive force ψ acts, and p is the impulsive fluid-pressure. Now generally there will be no impulsive forces acting bodily on the fluid. But the velocity will be generated by the impulsive pressures only; and therefore, if σ does not satisfy $V\nabla\sigma=0$, it must be on account of one of two reasons: either during the impulse d does not follow the law of dependence on p, which is highly probable; or p is discontinuous, so that the form ∇p is an improper form.

IV.

The velocity can also be written in a form of simple appearance, thus,

$$\sigma = -\frac{\dot{\varphi}_1}{\mathrm{H}}\alpha - \frac{\dot{\varphi}_2}{\mathrm{H}}\beta - \frac{\dot{\varphi}_3}{\mathrm{H}}\gamma, \quad . \quad . \quad . \quad . \quad (1)$$

since

$$\mathrm{D}_t\dot{\varphi}_1 = \dot{\varphi}_1 - (\mathrm{S}\sigma\nabla\varphi_1) = 0,$$

whence

$$\mathrm{D}_t\sigma = -\mathrm{D}_t\dot{\varphi}_1\frac{\alpha}{\mathrm{H}} - \mathrm{D}_t\dot{\varphi}_2\frac{\beta}{\mathrm{H}} - \mathrm{D}_t\dot{\varphi}_3\frac{\gamma}{\mathrm{H}}$$
$$-\left(\dot{\varphi}_1\frac{d}{d\varphi_1} + \dot{\varphi}_2\frac{d}{d\varphi_2} + \dot{\varphi}_3\frac{d}{d\varphi_3}\right)\sigma.$$

But

$$(\mathrm{S}\sigma\nabla) = \dot{\varphi}_1\frac{d}{d\varphi_1} + \dot{\varphi}_2\frac{d}{d\varphi_2} + \dot{\varphi}_3\frac{d}{d\varphi_3};$$

$$\therefore\ d_t\sigma = -\mathrm{D}_t\dot{\varphi}_1\frac{\alpha}{\mathrm{H}} - \mathrm{D}_t\dot{\varphi}_2\frac{\beta}{\mathrm{H}} - \mathrm{D}_t\dot{\varphi}_3\frac{\gamma}{\mathrm{H}},$$

and the condition for steady motion is $\mathrm{D}_t\dot{\varphi} = 0$, showing a close resemblance in form between the velocity in this case and the rotation in a perfect fluid *.

We may at any time, by elimination of t, find two surfaces of the nature φ for which $\dot{\varphi} = 0$; and since in steady motion $\mathrm{D}_t\dot{\varphi} = 0$, the property will continue in the surface. Taking the two such surfaces for φ_2 and φ_3, they will act as fixed boundaries, their intersections will be the stream-lines, and the form of σ will be

$$\sigma = -\frac{\dot{\varphi}_1}{\mathrm{H}}\alpha. \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

* [Note that $\dot{\varphi}_1$ is the flux through the side φ_1 of the element of the fluid under consideration, and remains unaltered if the motion is steady. If also $\mathrm{S}\nabla\sigma \doteq 0$, $\dot{\varphi}_1$ has properties similar to those of Σ_1.]

In spite of the simplicity of this form, it does not appear to yield a convenient form of condition affecting $D_t\sigma$, nor for $V\nabla\sigma$. The following property, however, can be deduced.

In a perfect fluid under conservative forces we must have $(S\sigma\nabla)\sigma$ of the form ∇P. But

$$(S\sigma\nabla)\sigma = \nabla\frac{\sigma^2}{2} + V\sigma V\nabla\sigma;$$

$$\therefore\ V\sigma V\nabla\sigma = \nabla Q, \text{ say,}$$

or

$$-\dot{\varphi}_1\bar{\Sigma}_3\nabla\varphi_2 + \dot{\varphi}_1\bar{\Sigma}_2\nabla\varphi_3 = \nabla Q,$$

whence

$$\frac{dQ}{d\varphi_1} = 0;$$

and $\frac{dQ}{d\varphi_2}$ and $\frac{dQ}{d\varphi_3}$ become 0 when acted on by D_t, whence Q is a function of φ_2 and φ_3 only. The existence of this surface Q, on which both stream-lines and vortex-lines lie, is dependent on the existence of vortex motion; but if the surface exists we may take it in place of the surface φ_3 to indicate the stream-lines; and then we get

$$\bar{\Sigma}_3 = 0$$

and

$$\dot{\varphi}_1\bar{\Sigma}_2 = 1,$$

or

$$\bar{\Sigma}_2 = \frac{1}{\dot{\varphi}_1},$$

and the rotation would be given by

$$V\nabla\sigma = \bar{\Sigma}_1 V\nabla\varphi_2\nabla Q + \frac{1}{\dot{\varphi}_1}V\nabla Q\nabla\varphi_1. \quad . \quad . \quad (3)$$

Steady vortex motion will, I think, generally occur in cases allied to the surfaces of discontinuity investigated by Helmholtz; and the surface Q will then be such a surface.

XVII. *The Literary History of Parnell's 'Hermit.'* By William E. A. Axon, M.R.S.L., &c.

Read December 28th, 1880.

Although Parnell's poem of the 'Hermit' can no longer be considered what Mitford declared it to be, "one of the most popular in our language," it still holds a certain and assured place in English literature. But, apart from its interest as a piece of English verse that has been a favourite with several generations, the 'Hermit' demands attention as one link in a curious chain of the history of fiction.

The readers of Voltaire are never likely to forget his romance of 'Zadig;' and one of the most striking passages in that remarkable work is the twentieth chapter, in which Zadig travels in company with an angel disguised as an hermit who steals a gold cup from a dispenser of ostentatious hospitality to give it to a miserly curmudgeon, burns down the house of a man who has received them with true liberality, and drowns the nephew of a widow lady by whom they had been most honourably entertained. These seemingly unjust and atrocious actions are all justified by the

wider view of the supernatural being who has read the book of fate and can foresee their real effect. The transfer of the cup is to reform the pride of the one and to excite the generosity of the other. Beneath the ruins of his wrecked mansion the good man finds a greater treasure to recompense his loss. The widow mourns the innocent youth of one who, if he had lived another year, would have been her murderer. Thus does the hermit vindicate the dark and mysterious ways of Providence to man.

Some of the critics, vain in the possession of a little learning, remarked that Voltaire's apologue was not original, but copied from Parnell. It is quite possible that such was the case; though Fréron might have remembered that Antoinette Bourignon, the mystic, had employed the same fable*. Parnell, although he does not make any avowal of his indebtedness to any previous author, would hardly have cared or dared to claim credit for the invention of the story. He found the fable ready to his hand; he saw that it formed good material for poetry; and accordingly he made the best use of it that he could in the poem which, more than any thing else, has kept his memory from oblivion. Pope says that Parnell found the story in Howell's 'Letters,' a very curious book which was first printed in 1645. Pope pronounced Parnell's poem very good. "The story," he says, "was written originally in Spanish, whence probably Howell had translated it into prose and inserted it in one of his 'letters.' Addison liked the scheme, and was not disinclined to come into it"†. O this supposed Spanish original we have no other testimony.

* Mitford has pointed this out in his 'Life of Parnell,' p. 61, where he quotes from W. Harte these two lines:—

> "Antonia, who the Hermit's story fram'd,
> A tale to prosemen known, by versemen famed."

She was born in 1616, and died in 1680.

† Goldsmith's 'Life of Parnell.'

James Howell found the story in Sir Percy Herbert's 'Certaine Conceptions or Considerations upon the Strange Change of People's Dispositions and Actions in these latter times,' a work "directed to his Sonne" and printed in the year 1652*. Yet Howell's 'Letters' were printed two years earlier, as Beloe has pointed out†. But as this apologue is the sixth letter in the fourth volume, it may have been added in a later issue.

It is also used by Henry More, the Platonist, in his 'Divine Dialogues,' which were published in 1668. The "Eremite and the Angel" is in the second dialogue, chap. xxiv., and follows very closely that given in the 'Gesta Romanorum,' to which we shall presently refer. This coincidence was pointed out by Mr. S. Whyte at the close of the last century‡ More's version is as follows:—

"A certain Eremite having conceived great jealousies touching the due administration of Divine Providence in external occurrences in the world, in this anxiety of mind was resolved to leave his cell and travel abroad to see with his own eyes how things went abroad in the world. He had not gone half a day's journey, but a young man overtook him and joyn'd company with him and insinuated himself so far into the Eremite's affection, that he thought himself very happy in that he had got so agreeable a companion. Wherefore resolving to take their fortunes together, they always lodged in the same house. Some few days' travels had overpast before the Eremite took notice of any thing remarkable. But at last he observed that his fellow-traveller, with whom he had contracted so intimate a friendship, in an house where they were extra-

* Lowndes, Bib. Man. (Bohn), p. 1049. Dunlop's 'History of Fiction,' 4th edit. (1876) p. 290.

† Beloe, 'Anecdotes of Literature,' vol. vi. (1812) p. 324. He gives the story in full from Herbert.

‡ 'Miscellanea Nova,' by S. and E. A. Whyte, (Dublin, 1800) p. 145.

ordinary well treated, stole away a gilt cup from the gentleman of the house and carried it away with him. The Eremite was very much astonished with what he saw done by so fair and agreeable a person as he conceived him to be; but thought not yet fit to speak to him or seem to take notice of it. And therefore they travel fairly on together as aforetimes, till night forced them to seek lodging. But they light upon such an house as had a very unhospitable owner, who shut them out unto the outward court and exposed them all night to the injury of the open weather, which chanced then to be very rainy; but the Eremite's fellow-traveller unexpectedly compensated his host's ill-entertainment with no meaner reward than the gilt cup he had carried away from the former place, thrusting it in at the window when they departed. This the Eremite thought was very pretty, and that it was not covetousness but humour that made him take it away from its first owner. The next night where they lodged they were treated again with a deal of kindness and civility: but the Eremite observed with horrour that his fellow-traveller for an ill requital strangled privately a young child of their so courteous host in the cradle. This perplext the mind of the poor Eremite very much; but in sadness and patience forbearing to speak, he travelled another day's journey with the young man, and at evening took up in a place where they were more made of than anywhere hitherto. And because the way they had to travel next morning was not so easie to find, the master of the house commanded one of the servants to go part of the way to direct them; whom, while they were passing over a stone bridge, the Eremite's fellow-traveller caught suddenly betwixt the legs and pitched him headlong from off the bridge into the river and drowned him. Here the Eremite could have no longer patience, but flew bitterly upon his fellow-traveller

for those barbarous actions, and renounced all friendship with him, and would travel with him no longer nor keep him company. Whereupon the young man smiling at the honest zeal of the Eremite, and putting off his mortal disguise, appeared as he was, in the form and lustre of an angel of God, and told him he was sent to ease his mind of the great anxiety it was encumbered with touching the Divine Providence. 'In which,' said he, 'nothing can occur more perplexing and paradoxical than what you have been offended at since we two travelled together. But yet I will demonstrate to you,' said he, 'that all that I have done is very just and right. For, as for that first man from whom I took the gilded cup, it was a real compensation indeed of his hospitality; that cup being so forcible an occasion of the good man's distempering himself and of hazarding his health and life, which would be a great loss to his poor neighbours, he being of so good and charitable a nature. But I put it into the window of that harsh and unhospitable man that used us so ill, not as a booty to him, but as a plague and a scourge to him, and for an ease to his oppressed neighbours, that he may fall into intemperance, disease, and death itself. For I knew very well that there was that enchantment in this cup, that they that had it would be thus bewitched with it. As for that civil person whose child I strangled in the cradle, it was in great mercy to him and no real hurt to the child, who is now with God. But if that child had lived, whereas this gentleman had been piously, charitably, and devotedly given, his mind, I saw, would have unavoidably sunk into the love of the world, out of love to his child, he having had none before, and doting so hugely on it; and therefore I took away this momentary life from the body of the child, that the soul of the father might live for ever. And for this last act, which you so much abhor, it was the most

faithful piece of gratitude I could do to one that had used us so humanely and kindly as that gentleman did. For this man, who, by the appointment of his master, was so officious to us as to show us the way, intended this very night ensuing to let in a company of rogues into his master's house to rob him of all that he had, if not to murder him and his family.' And having said thus, he vanished. But the poor Eremite, transported with joy and amazement, lift up his hands and eyes to heaven and gave glory to God who had thus unexpectedly delivered him from any farther anxiety touching the ways of Providence, and thus returned with cheerfulness to his forsaken cell and spent the residue of his days there in piety and peace."

Indeed, in the seventeenth century it had become a commonplace with which theologians might "point a moral or adorn a tale." Thus Thomas White, a Puritan divine, writing in 1658, says:—

"There is a famous story of Providence in *Bradwardine* to this purpose:—A certain Hermit that was much tempted and was much unsatisfied concerning the providence of God, resolved to journey from place to place till he met with some that could satisfie him. An Angel in the shape of a man joyned himself with him as he was journeying, telling him that he was sent from God to satisfie him in his doubts of providence. The first night they lodged at the house of a very holy man, and spent their time in discourses of heaven and praises of God, and were entertained with a great deal of freedom and joy. In the morning when they departed the Angel took with him a great cup of gold. The next night they came to the house of another holy man who made them very welcome and exceedingly rejoyced in their society and discourse; the Angel notwithstanding, at his departure, kill'd an infant in the cradle, which was his only son, being many years

before childless, and therefore was a very fond father of this child. The third night they came to another house where they had like free entertainment as before. The master of the family had a steward whom he highly prized, and told them how happy he accounted himself in having such a faithful servant. Next morning he sent this his steward with them part of their way to direct them therein: as they were going over a bridge the Angel flung the steward into the river and drowned him. The last night they came to a very wicked man's house, where they had very untoward entertainment; yet the angel next morning gave him the cup of gold. All this being done, the Angel asked the Hermit whether he understood those things. He answered his doubts of Providence were increased, not resolved; for he could not understand why he should deal so hardly with those holy men who received them with so much love and joy, and yet give such a gift to that wicked man who used them so unworthily. The angel said, 'I will now expound these things unto you. The first house where we came the master of it was a holy man, yet drinking in that cup every morning, it being too large, it did somewhat unfit him for holy duties, though not so much that others or himself did perceive it; so I took it away, since it is better for him to loose the cup of gold than his temperance. The master of the family where we lay the second night was a man given much to prayer and meditation, and spent much time in holy duties, and was very liberal to the poor, all the while he was childless; but as soon as he had a son he grew so fond of it, spent so much time in playing with it that he exceedingly neglected his former holy exercise and gave but little to the poor, thinking he could never lay up enough for his childe; therefore I have taken the infant to Heaven and left him to serve God better upon Earth. The steward whom I did drown had

plotted to kill his master the night following. And as for that wicked man to whom I gave the cup of gold, he was to have nothing in the other world, I gave him something in this which, notwithstanding, will prove a snare to him, for he will be more intemperate; and let him which is filthy be more filthy.' The truth of this story I affirm not; but the moral is very good; for it shows that God is an indulgent father to the saints when he most afflicts them, and that when he sets the wicked on high 'he sets them also in slippery places, and their prosperity is their ruine.'—Prov. i. 32"*.

The caution of the worthy divine is to be commended in declining to affirm the literal truth of this narrative.

White, it will be noticed, gives Bradwardine as the authority for this apologue. This may be conjectured to be the author who was styled the Doctor profundus and whose 'Causa Dei contra Pelagium' was a work of weight and fame in the fourteenth century†. He was an Archbishop of Canterbury, who was born in 1290 or earlier, and died in 1349, of the plague. We can thus trace the legend in England to the early part of the fourteenth century.

In Germany it was used by Luther and by Joh. Herolt‡, whose 'Sermones de Tempore' were printed at Nuremberg in 1496.

In the thirteenth century it is found in several forms. From M. Gaston Paris § we learn that it is in the sermons

* White's (Th.) 'Treatise of the Power of Godliness,' 1658, pp. 376–379.

† Hook's 'Lives of the Archbishops of Canterbury,' vol. iv. (1865) p. 80.

‡ Mitford's 'Life of Parnell,' prefixed to the Aldine edition of that poet.

§ "L'Ange et l'Ermite, étude sur une légende religieuse, par Gaston Paris, lue dans la séance publique annuelle de l'Académie des Inscriptions, 12 Nov. 1880," *Journal Officiel*, 16 Nov. 1880. The present paper was in progress before the appearance of the "étude" of M. Paris. All special indebtedness to his work has been carefully acknowledged.

of Jacques de Vitri, who died in 1240, and in the 'Scala Cœli' of Jean le Jeune, who wrote about the commencement of the fourteenth century. "This beautiful apologue," observes Mr. Thomas Wright, "is of frequent occurrence in old MSS., and differs considerably in different copies." He has printed a Latin version from the Harleian MSS. of the thirteenth or fourteenth century*. The great collection of stories known as the 'Gesta Romanorum,' there is reason to suppose, was compiled in England about the close of the thirteenth century for the use of preachers. It has been a storehouse for the poets and dramatists also; but its original intention was to provide the ecclesiastics with something wherewith to enliven their dry theological discourses. The story of the Hermit and the Angel is the eightieth of this collection; and an abstract of it is given by Warton†.

The story is found in a French conte, published in 1823, by Méon, who found it added to some of the manuscripts of the 'Vie des Pères,' to which it did not originally belong. In this poem we have the incidents of a cup stolen from one host and given to another, of the servant drowned, of the infant strangled, and of an abbey burned down that the monks might once more be poor and pious. By a process of natural selection Voltaire has omitted one of the murders, and Parnell has left out the conflagration. From this it may be doubted whether the witty Frenchman was indebted to the English poet or to one of the earlier texts. This has also been commented upon by Dunlop‡.

The story is also in some of the recensions of the 'Vitæ

* 'Latin Stories,' edited by T. Wright, 1842, pp. 10 and 247.

† Warton, Hist. of English Poetry, edited by Hazlitt (1871), vol. i. p. 256.

‡ Dunlop, 'History of Fiction,' 4th edit. 1876, p. 289. Wright's 'Latin Stories,' 1842, p. 101.

Patrum.' One of these, in the 'Bibliothèque Mazarine,'' which has been published by M. E. du Méril, is regarded by M. Gaston Paris as the origin of the mediæval variants. In this manuscript of the fourteenth century the actors in the story are all hermits or ecclesiastics, but the incidents, with the exception of the fire, are the same.

Goldsmith, writing of Parnell's 'Hermit,' says that he had been told that the fable was an Arabian invention. In effect it is in the Koran, where Moses is said to have met a nameless prophet whom the commentators style Al-Khedr :—

"And Moses said unto him, 'Shall I follow thee that thou mayest teach me part of that which thou hast been taught for a direction unto me?' He answered, 'Verily thou canst not bear with me: for how canst thou patiently suffer those things the knowledge whereof thou dost not comprehend?' Moses replied, 'Thou shalt find me patient if God please, neither will I be disobedient unto thee in any thing.' He said, 'If thou follow me, therefore, ask me not concerning any thing until I shall declare the meaning thereof unto thee.' So they both went on by the sea-shore, until they went up into a ship; and he made a hole therein. And Moses said unto him, 'Hast thou made a hole therein that thou mightest drown those who are on board? Now hast thou done a strange thing.' He answered, 'Did I not tell thee thou couldest not bear with me?' Moses said, 'Rebuke me not, because I did forget, and impose not on me a difficulty in what I commanded.' Wherefore they left the ship and proceeded until they met with a youth; and he slew him. Moses said, 'Hast thou slain an innocent person without his having killed another? Now hast thou committed an unjust action.' He answered, 'Did I not tell thee that thou couldest not bear with me?'

Moses said, 'If I ask thee concerning any thing hereafter, suffer me not to accompany thee. Now hast thou received an excuse from me.' They went forwards, therefore, until they came to the inhabitants of a certain city: and they asked food of the inhabitants thereof; but they refused to receive them. And they found therein a wall which was ready to fall down; and he set it upright. Whereupon Moses said unto him, 'If thou wouldest, thou mightest have received a reward for it.' He answered, 'This shall be a separation between me and thee: but I will first declare unto thee the signification of that which thou couldest not bear with patience. The vessel belonged to certain poor men who did their business in the sea; and I was minded to render it unserviceable because there was a king behind them who took every sound ship by force. As to the youth, his parents were true believers, and we feared lest he, being an unbeliever, should oblige them to suffer his perverseness and ingratitude: wherefore we desired that their Lord might give them a more righteous child in exchange for him, and one more affectionate towards them. And the wall belonged to two orphan youths in the city, and in it was a treasure hidden which belonged to them; and their father was a righteous man: and thy Lord was pleased that they should attain their full age, and take forth their treasure, through the mercy of thy Lord. And I did not what thou hast seen of my own will, but by God's direction. This is the interpretation of that which thou couldest not bear with patience' "*.

This is the oldest literary form of Parnell's 'Hermit.' It may well be supposed that the Arabian Prophet borrowed the beautiful legend, as he did many other things, from a

* Koran, Sale's translation, chap. xviii. Dunlop's 'History of Fiction,' p. 292.

Jewish source. The Talmud may, in its present form, be later than the Koran; but it embodies the traditions of a race who have always clung to the sacred memories of their literature and their religion. The form in which we find it in this vast encyclopedia of Hebrew learning is very different from those already given:—

"Rabbi Jochanan, the son of Levi, fasted and prayed to the Lord that he might be permitted to gaze on the angel Elijah, he who had ascended alive to heaven. God granted his prayer; and in the semblance of a man Elijah appeared before him.

"'Let me journey with thee in thy travels through the world,' prayed the Rabbi to Elijah; 'Let me observe thy doings, and gain in wisdom and understanding.'

"'Nay,' answered Elijah; 'my actions thou couldst not understand; my doings would trouble thee, being beyond thy comprehension.'

"But still the Rabbi entreated. 'I will neither trouble nor question thee,' he said; 'only let me accompany thee on thy way.'

"'Come then,' said Elijah; 'but let thy tongue be mute. With thy first question, thy first expression of astonishment, we must part company.'

"So the two journeyed through the world together. They approached the house of a poor man whose only treasure and means of support was a cow. As they came near, the man and his wife hastened to meet them, begged them to enter their cot and eat and drink of the best they could afford, and to pass the night under their roof. This they did, receiving every attention from their poor but hospitable host and hostess. In the morning Elijah rose up early and prayed to God, and when he had finished his prayer, behold the cow belonging to the poor people dropped dead.

"Then the travellers continued on their journey.

"Much was Rabbi Jochanan perplexed. 'Not only did we neglect to pay them for their hospitality and generous services, but his cow we have killed;' and he said to Elijah, 'Why didst thou kill the cow of this good man who ——'

"'Peace!' interrupted Elijah; 'hear, see, and be silent! If I answer thy questions we must part.' And they continued on their way together.

"Towards evening they arrived at a large and imposing mansion, the residence of a haughty and wealthy man. They were coldly received; a piece of bread and a glass of water were placed before them, but the master of the house did not welcome or speak to them, and they remained there during the night unnoticed. In the morning Elijah remarked that a wall of the house required repairing, and sending for a carpenter, he himself paid the money for the repair as a return, he said, for the hospitality they had received.

"Again was Rabbi Jochanan filled with wonder; but he said naught, and they proceeded on their journey.

"As the shades of night were falling, they entered a city which contained a large and imposing synagogue. As it was the time of the evening service, they entered and were much pleased with the rich adornments, the velvet cushions, and gilded curves of the interior. After the completion of the service, Elijah arose and called out aloud, 'Who is here willing to feed and lodge two poor men this night?' None answered, and no respect was shown to the travelling stranger. In the morning, however, Elijah reentered the synagogue, and, shaking its members by the hands, he said, 'I hope that you may all become presidents.'

"Next evening the two entered another city, when

the *Shamas* (sexton) of the synagogue came to meet them, and notifying the members of his congregation of the coming of two strangers, the best hotel of the place was opened to them, and all vied in showing them attention and honour.

"In the morning, on parting with them, Elijah said, 'May the Lord appoint over you but one president.'

"Jochanan could resist his curiosity no longer. 'Tell me,' said he to Elijah, 'tell me the meaning of all these actions which I have witnessed. To those who have treated us coldly thou hast uttered good wishes; to those who have been gracious to us thou hast made no suitable return. Even though we must part, I pray thee explain to me the meaning of thy acts.'

"'Listen,' said Elijah, 'and learn to trust in God, even though thou canst not understand His ways. We first entered the house of the poor man who treated us kindly. Know that it had been decreed that on that very day his wife should die. I prayed unto the Lord that the cow might prove a redemption for her; God granted my prayers, and the woman was preserved unto her husband. The rich man whom next we called up, treated us coldly, and I repaired his wall. I repaired it without a new foundation, without digging to the old one. Had he repaired it himself, he would have dug and thus discovered a treasure which lies there buried, but which is now for ever lost to him. To the members of the synagogue who were inhospitable, I said, 'May you all be presidents,' and where many rule there can be no peace; but to the others I said, 'May you have but one president;' with one leader no misunderstanding may arise. Now, if thou seest the wicked prospering, be not envious; if thou seest the righteous in poverty and trouble, be not provoked or doubtful of God's justice. The Lord is righteous, His

judgments all are true; His eyes note all mankind, and none can say, 'What dost thou?'"

"With these words Elijah disappeared, and Jochanan was left alone"*.

There is another story illustrating the same moral. "Moses sees a warrior come to a fountain, by whose side he leaves a sack of gold, which was taken away by a shepherd. An old man, bending beneath a heavy burden, then came to the fountain, when the horseman returned and accused him of having purloined the sack of gold. In spite of his protestations of innocence the warrior drew his sword and slew the old man. Whilst Moses is filled with horror at the sight, the voice of God explains to him that the old man had murdered the father of the warrior, that the money really belonged to the shepherd, although he was unaware of it, and that the warrior lost because he had acquired it without right and used it only for evil purposes"†.

This has also found its way into the 'Gesta Romanorum' and similar collections.

We have thus traced Parnell's 'Hermit' as far back as is at present possible. Whether it was the invention of a Jewish poet or borrowed by a Hebrew moralist from some still earlier source it is impossible to say.

That the Prophet of Islam learned it from some of the Arabian Jews is very probable; but the manner in which it entered Europe and the mode in which it became incorporated with the ecclesiastical literature of the middle ages are not known; though M. Paris has conjectured that

* 'The Talmud,' by H. Polano, (London, n. d.) p. 313. Baring-Gould's 'Legends of Old-Testament Characters,' vol. ii. (1871) p. 113.

† Baring-Gould's 'Legends of Old-Testament Characters,' vol. ii. (1871) p. 113.

it may have come from Egypt, where adherents of the three faiths of Judaism, Islam, and Christianity existed side by side. In corroboration of this, the simplest form of the European story has for its characters the hermits of the Thebaid.

The apologue commended itself not only to a crowd of churchmen and divines, but to a poet like Parnell, a fanatic like Antoinette Bourignon, and a doubter like Voltaire. Sometimes it assumes the form of a very practical homily upon everyday life, and at others is bounded by the narrow limits of the artificial virtues of ecclesiasticism; but in each case the motive is the same. All versions of the legend seek to vindicate the moral order of the universe by an explanation of the seeming contradiction of particular instances.

The problems of life are essentially the same in all ages. "I have been young," says the Psalmist, "and now am old; yet have I not seen the righteous forsaken, nor his seed begging their bread." There are many, however, both in ancient and modern days, who have not been so fortunate, and who have looked out upon a world where the righteous, to all earthly appearance, were forsaken. They have seen the tyrant triumphant whilst none dared to comfort the slave. They have seen Vice seated on the throne and Virtue dying in the dungeon. They have seen sorrow and evil in a thousand forms.

The existence of evil is alike the moral and physical riddle of the universe. Notwithstanding all man's efforts the sphinx has not relaxed the rigidity of her features, which still proclaim her the keeper of the unsolved mystery. This beautiful Hebrew apologue is one of the many efforts to reconcile the conception of an all-good and all-wise ruler of the universe with the existence of Wrong clothed in purple and fine linen, and of Right

that eats the bread of sorrow and drinks the water of affliction.

There is a subtler problem which the story leaves untouched. It deals only with the surface of things. Beautiful as it is, it embodies the judgment of a primitive people who see only the concrete aspects of life. With them the blessings of God take visible shape in worldly possessions, in flocks and herds, in gold and silver, in menservants and maidservants. The real touchstone, however, is internal, and not external.

> "He that has light within his own clear breast
> May sit i' the centre and enjoy bright day;
> But he that hides a dark soul and foul thoughts
> Benighted walks under the mid-day sun;
> Himself is his own dungeon.

Into this sphere of thought the old fabulist enters not. He is content to give dramatic force to that which Pope has expressed in didactic form :—

> "All Nature is but art unknown to thee;
> All chance, direction which thou canst not see;
> All discord, harmony not understood;
> All partial evil, universal good;
> And spite of pride, in erring reason's spite,
> One truth is clear, whatever is is right.

XVIII. *On the Long-period Inequality in Rainfall.* By Balfour Stewart, LL.D., F.R.S., Professor of Natural Philosophy at the Owens College, Manchester.

Read February 24th, 1880.

1. If it be true that there is a variation in the power of the sun depending on the state of his surface, this variation might naturally be expected to make itself apparent through a corresponding change in the rainfall of the earth; so that when the sun is most powerful there ought to be the greatest rainfall.

2. While the connexion indicated above is that which most readily occurs to the mind, yet the difficulty of ascertaining the facts of the case in a manner bearing the smallest approach to completeness is so great as to be at present insuperable.

There is *first of all* an intense reference to locality in rainfall, so that the rainfall at one place may differ greatly from that at another place in its near neighbourhood. *Again,* there are probably, in addition to possible secular inequalities, very great oscillations in the yearly rainfall at any one place, or accidental variations, as we may term them in our ignorance of their cause.

Thirdly, it is in comparatively few places, and those places chosen without the smallest reference to this particular problem, that we have any thing like a trustworthy account of the rainfall throughout a considerable number of years.

Fourthly, we have no information of any importance with respect to the rainfall at sea.

3. Besides the formidable catalogue of difficulties now mentioned, we ought to bear in mind the following considerations. The convection currents of the earth are regulated by two things, one of which is constant, while the other may be variable. The constant element is the velocity of rotation of the earth on its axis, while the element of possible variability is the power of the sun. Hence it follows that, if the sun be variable, it will cause a variation in the direction as well as in the intensity of the earth's convection currents, on the principle which tells us that the resultant of two forces, one constant and the other variable, must vary both in magnitude and direction.

Now, if it be true that we have a long-period variation, not merely of the intensity, but also of the distribution of the earth's convection currents, and if we bear in mind the intensely local reference in rainfall, it would be too much to expect that the rainfall inequality should exhibit the same years of maximum and minimum at all places. It is even conceivable that some places might exhibit a maximum while others showed a minimum, while others, again, might exhibit a double instead of a single period.

4. It appears to me that, if we bear in mind these considerations, it will not answer to add together the rainfall of a few selected stations as they stand, with the view of determining by this means whether there be a long-period inequality in the rainfall of the *whole Earth.* We are not yet in a position to reply experimentally to this question. It does not, however, follow that nothing can be done. Dr. Meldrum and others appear to have achieved good preliminary work in the direction of indicating the exist-

ence of a rainfall-inequality depending upon the state of the sun. Dr. Meldrum began by pointing out that in a good many places there is a greater rainfall during years of maximum than during years of minimum sun-spots, and that this phenomenon repeats itself from one solar cycle to another. Again, Governor Rawson has pointed out the existence of certain localities where the rainfall-inequality appears to be of a precisely opposite character, while Dr. Hunter has showed the practical importance of the investigation with reference to certain tropical stations. The subject has likewise been discussed by Smyth, Stone, and others.

5. The question has arisen whether it might be possible to throw any light on this problem by the method of detecting unknown inequalities, proposed by Mr. Dodgson and myself (see 'Proceedings' of the Royal Society, May 29, 1879). The essence of this method consists in a way by which we may numerically estimate the indications of an inequality. Let us suppose for instance that, in ignorance of the diurnal range of temperature, we try to find whether there be a temperature-inequality of 24 hours or whether there be not rather one of 26 hours. We should begin by taking a large number of hourly readings of temperature; and we should group these into two series, the one containing 24 numbers in each horizontal row, and the other 26. We should thus have 24 vertical columns from the one series and 26 from the other; and we should take the mean of each vertical column of each series as well as the mean of the whole.

Now it would speedily be found that an inequality was indicated by the 24-hourly series and none by that of 26 hours. For in the first series the mean of the vertical column representing observations at 5 A.M. would be greatly less than the mean of the whole, while the mean

of the column representing observations at 2 P.M. would be much higher than the mean of the whole. On the other hand, in the 26-hourly series, provided it were sufficiently extensive, we should perceive no such differences. Thus, in the 24-hourly series the differences of the means of the various vertical columns from the mean of the whole would be much greater than in the 26-hourly series; and the mean amount of these differences might be taken to form a numerical criterion of the presence or absence of an inequality.

6. This method, therefore, applied to the subject in hand, might be expected to reveal the presence or absence of inequalities in rainfall, provided we have observations sufficient for the purpose. It is clear that the successful application of this method does not require a previous knowledge of the exact form of the inequality. Whether a maximum rainfall occurs at epochs of maximum or at epochs of minimum sun-spot frequency, whether there be only one rainfall maximum corresponding to the solar period, or two, or even three, is a matter of no consequence as far as this method is concerned. All that is necessary is that the rainfall should always be similarly affected by similar states of the sun. Here, however, we must bear in mind that this method of detecting inequalities by summing up and averaging the departures from the mean caused by the inequality, likewise sums up and averages the accidental fluctuations. Now these accidental fluctuations are particularly large for rainfall; and it is therefore desirable to lessen their disturbing effect as much as possible. This can only be done by confining ourselves to long series of observations, in which the accidental fluctuations may be supposed to counteract each other to a great extent, while the long-period fluctuations will remain behind.

7. Through the kindness of Mr. Whipple, Director of the Kew Observatory, I have received copies of those catalogues of rainfall which he has himself made use of in a paper which was recently communicated to the Royal Society (January 8, 1880). Of these Paris, Padua, England, and Milan form the most extensive series, that of Paris embracing 161 years, Padua 154, England (Symons's Table) 140, Milan 115. Mr. Whipple has likewise furnished materials by which the labour of applying the process in hand to these series will be much abridged; and he has kindly allowed me to make use of these. I will therefore apply the process to these four stations.

8. Let us begin by grouping the Paris yearly values into series of 8. We thus obtain the following final numbers expressed in centimetres—51·4, 47·5, 45·7, 48·7, 51·1, 49·8, 46·5, 47·2, the mean being 48·5. From these we obtain the following series of differences:—

$$+2{\cdot}9,\ -1{\cdot}0,\ -2{\cdot}8,\ +0{\cdot}2,\ +2{\cdot}6,\ +1{\cdot}3,\ -2{\cdot}0,\ -1{\cdot}3.$$

In order to diminish the effect of accidental fluctuations, let us equalize this series of differences by taking the mean of each two. We thus obtain—

$$+0{\cdot}8,\ +1{\cdot}0,\ -1{\cdot}9,\ -1{\cdot}3,\ +1{\cdot}4,\ +1{\cdot}9,\ -0{\cdot}4,\ -1{\cdot}7.$$

If we now add these together, *without respect of sign*, and divide by their number (8), we obtain 1·3 as the mean departure from the mean of the whole; and bringing this into a proportional shape by dividing it by the mean rainfall (48·5), we obtain $\frac{1{\cdot}30}{48{\cdot}5} = 2{\cdot}68$ per cent.

9. These explanations will enable the reader at once to perceive the principle of construction of the following Table:—

Proportional Rainfall-inequality, as exhibited by series of years.

	Eight years.	Nine years.	Ten years.	Eleven years.	Twelve years.	Thirteen years.	Fourteen years.
English rainfall, Symons's Catalogue	2·63	2·14	1·55	1·79	3·15	1·69	2·57
Paris	2·68	3·07	1·99	2·65	3·70	2·57	3·08
Padua	1·77	3·62	2·02	1·47	3·31	3·52	3·40
Milan	1·12	3·22	3·16	1·78	4·13	3·78	2·49

We ought to give the English, the Paris, and the Padua observations a somewhat higher weight than those of Milan, as the former embrace a longer period. This will be done sufficiently well by giving the first three sets weights of 3 each and the Milan set a weight of 2. If we perform this operation and then take the mean, we obtain as under :—

	Eight years.	Nine years.	Ten years.	Eleven years.	Twelve years.	Thirteen years.	Fourteen years.
Mean of the four stations, weighted as above ...	2·15	3·00	2·09	1·94	3·52	2·81	2·92

A maximum corresponding to nine years, and a still greater one, corresponding to twelve years, are thus exhibited, each of these being recorded at three stations out of four.

The proportional numbers indicated are not large; but it must be remembered that it is the mean difference for all the years that is given, and that the maximum and minimum rainfall will represent differences above and below the mean which will each be about double the numbers recorded above.

10. Regarding the rainfall-values as representing the meteorological result of the sun's action, let us now compare these with declination-range values, which may be taken to represent the sun's magnetic effect. Professor Loomis has compiled (American Journal of Science and

Arts, 2nd ser. vol. l. p. 153) what seems to be a very good Table, exhibiting a set of yearly values of magnetic declination-range extending, with slight breaks, from 1777 to 1868.

Let us take this Table and treat it precisely as we have treated the rainfall, except that it does not seem necessary to make any attempt at equalization such as that made in article 8.

We thus obtain the following result:—

Proportional Declination-range Inequality, as exhibited by series of years.

	Eight years.	Nine years.	Ten years.	Eleven years.	Twelve years.	Thirteen years.	Fourteen years.
Prague, or reduced to Prague	3·37	3·39	10·07	4·66	9·33	4·09	4·98

Here we have decided maxima corresponding to ten and twelve years. The result is thus not unlike that which has been derived from rainfall-observations, where the maxima correspond to nine and twelve years; indeed we could hardly expect a more perfect correspondence between the two, bearing in mind the limited amount of observations which we have for determining inequalities of long periods.

Note added on March 6th.

I take this opportunity of saying a few words on what I imagine to be the proper line of policy that should be pursued in this research.

(1) There are manifestly two stages in the investigation. *In the first place* we wish to ascertain whether there is any connexion between the state of the sun's surface (as revealed by spots) and the meteorology of the earth; and

in the second place we wish to find the nature and laws of this connexion should it be proved to exist.

(2) If the various meteorological elements at the various stations of the earth are found to present the same periodic inequalities as those which characterize sun-spots, this must be taken as decisive in favour of a connexion of some sort between the two, quite irrespective of the exact form of the inequalities. Nor will this evidence be invalidated if an inequality at one station should be different in form from that at another.

(3) Assuming the probability (from the evidence already brought forward) of such a connexion, the most natural hypothesis is that which supposes that the sun has inequalities which affect his radiating-power. Hence it is of great importance (as proposed by Professor Stokes and others) to ascertain by judicious actinometrical experiments whether the heating effect of the sun's rays be in reality variable.

(4) In absence of actinometrical results, we have grounds for believing that the magnetic activity of the sun is greatest at epochs of maximum sun-spots; and it seems most natural that the meteorological activity of our luminary should be greatest when his magnetical activity is greatest.

From the reasoning of the paper to which this note is added we may conclude that there is no evidence which can be deduced from rainfall against this hypothesis.

(5) But while there is considerable preliminary evidence in favour of a variability in the heating-power of the sun, and while this is constantly accumulating, we must not deem it impossible that the sun affects the earth in some other way.

There is ground for supposing that the moon affects both the magnetism and meteorology of the earth in a way

which we do not at present understand; and it is possible that the sun may have a similar influence.

Since writing the above, I have learned that Mr. Baxendell made use of the method of mean departures described in this communication in one of a remarkable series of papers which he presented to this Society on March 8th, 1864. But I have no reason for supposing that he was aware of the peculiar characteristics of the method devised by Mr. Dodgson and myself, in virtue of which we can, with comparatively little trouble, ascertain the exact periods of inequalities which are crowded very near together in the time-scale.

XIX. *On a Form of representing the Velocity at any Point of an Incompressible Fluid under Conservative Forces.* By R. F. Gwyther, M.A.

Read February 24th, 1880.

1. The velocity at any point of a fluid may be represented in other forms than the usual velocity potential or the vector potential of Helmholtz.

The form $\sigma = \nabla\phi$ corresponds to the case when $S\sigma\delta\tau$ is a complete differential without a factor; let us imagine it to be made a complete differential by a factor—that is, σ to be of the form

$$\sigma = k\nabla\psi,$$

or to be a combination of the two, thus,

$$\sigma = \nabla\phi + k\nabla\psi. \quad \text{(I.)}$$

2. First let us consider the circumstances accompanying the forms. If $S\sigma\delta\tau$ is integrable by a factor, the condition is

$$S\sigma\nabla\sigma=0, \text{ or } S\sigma\rho=0,\text{—}$$

that is, the axes of vortex-motion perpendicular to the lines of flow, a case satisfying the conditions of parallel cylindrical vortices and vortex rings.

If $\sigma-\nabla\phi$ be integrable by a factor, the condition is

$$S(\sigma-\nabla\phi)\nabla(\sigma-\nabla\phi)=0,$$

or

$$S(\sigma-\nabla\phi)\rho=0.$$

From this scalar equation ϕ could be found; and we may consider $\sigma=\nabla\phi+k\nabla\psi$ as a general form of expression for the velocity at any point.

3. The kinematical condition that σ must satisfy is the "equation of continuity," which if the fluid is incompressible takes the form

$$S\nabla\sigma=0,$$

or

$$\nabla^2\phi+k\nabla^2\psi+S\nabla k\nabla\psi=0; \quad . \quad . \quad . \quad . \quad \text{(II.)}$$

and the angular velocity at any point in the fluid is given by

$$2\rho=V\nabla k\nabla\psi. \quad . \quad . \quad . \quad . \quad . \quad \text{(III.)}$$

The form taken by the equation of motion, when this expression is submitted for σ, will now be found; for

$$D_t\sigma=-\nabla\left(V+\frac{p}{m}\right)$$

may be written

$$D_t(\nabla\phi+k\nabla\psi)=-\nabla\left(V+\frac{p}{m}\right),$$

or

$$D_tk\nabla\psi+D_t\nabla\phi+kD_t\nabla\psi=-\Delta\left(V+\frac{p}{m}\right).$$

Now $D_t\nabla=\nabla\,.\,D_t-\Delta D_t$, where Δ acts only on the σ in D_t or $d_t-(S\sigma\nabla)$; therefore

$$\mathrm{D}_t \cdot \nabla\phi + k\mathrm{D}_t \cdot \nabla\psi = \nabla \cdot \mathrm{D}_t\phi + k\nabla \cdot \mathrm{D}_t\psi$$
$$+ \Delta\{(\mathrm{S}\sigma\nabla)\phi + k(\mathrm{S}\sigma\nabla)\psi\}$$
$$= \nabla \cdot \mathrm{D}_t\phi + k\nabla \cdot \mathrm{D}_t\psi + \tfrac{1}{2}\nabla\sigma^2,$$

and, finally, the equation of motion is

$$\delta_t\nabla\mathrm{D}_t\phi + \mathrm{D}_t k \cdot \nabla\psi + k\nabla \cdot \mathrm{D}_t\psi + \tfrac{1}{2}\nabla\sigma^2 = -\nabla\left(\mathrm{V} + \frac{p}{m}\right).$$

Now this equation is equivalent to three scalar equations; and if we make in it the substitutions $\mathrm{D}_t k = 0$, $\mathrm{D}_t\psi = 0$, we reduce it to a single equation and solvable; for then

$$\nabla\mathrm{D}_t\phi = -\nabla\left(\mathrm{V} + \frac{p}{m} + \frac{\sigma^2}{2}\right),$$

or

$$\mathrm{D}_t\phi = -\mathrm{V} - \frac{p}{m} - \frac{\sigma^2}{2} + \nabla^{-1}0,$$

where $\nabla^{-1}0$ is independent of the position and may be included in the ϕ on solution.

Putting for $\mathrm{D}_t\phi$, $\dot{\phi} - (\mathrm{S}\sigma\nabla)\phi$, and substituting for σ we get

$$\dot{\phi} - (\Delta\phi)^2 - k\mathrm{S}\nabla\phi\nabla\psi = -\mathrm{V} - \frac{p}{m} - \frac{(\nabla\phi + k\nabla\psi)^2}{2},$$

or

$$2\dot{\phi} - (\nabla\phi)^2 + k^2(\nabla\psi)^2 = -2\mathrm{V} - \frac{2p}{m}. \quad . \quad . \quad . \quad . \quad (\mathrm{IV.})$$

In support of the substitutions $\mathrm{D}_t k = 0$ and $\mathrm{D}_t\psi = 0$ I should state

(1) That such surfaces can be found from the differential equation.

(2) That only three scalar equations have been used in determining σ so as to satisfy the equation of motion.

(3) That as the intersections of such surfaces, if they exist, are to move with the fluid, it is not unnatural to

make the trial of the possibility; and a fourth we shall see later.

[It may not be out of place to notice that from the equation

$$\dot{\sigma}-(S\sigma\nabla)\sigma=\nabla\left(V+\frac{p}{m}\right),$$

which may be written

$$\dot{\sigma}-2V.\sigma\rho=\nabla\left(V+\frac{p}{m}+\frac{\sigma^2}{2}\right),$$

we may by operating with $S\nabla$ get the equation

$$4\rho^2-2S\nabla\rho\sigma=\nabla^2 P,$$

where P stands for $V+\frac{p}{m}+\frac{\sigma^2}{2}$,

an equation I have never seen stated.]

4. In order to interpret as far as possible the expressions here introduced, we take first the last two conditions, which express that the surfaces k and ψ move with the fluid so as always to contain the same fluid-elements; and, referring to the expression for the angular velocity, we see that they intersect in the vortex-lines.

It would be well to determine these surfaces more fully. We have as yet treated them as distinct. However, the surfaces k and ψ coincide or do not exist where vortex-motion does not exist; for then $V.\nabla k\nabla\psi=0$ at all points, and the normals to the surfaces at all points are parallel; whence the surfaces $k=$ const. and $\psi=$ const. coincide except at points where vortex-motion exists. In order, therefore, to apply this method to the solution of rotational motion of a fluid, we should consider $k\nabla\psi$ an additive term, and take for k and ψ values such as to make the surfaces $k=$ const. and $\psi=$ const. move with the rotational fluid, and always intersect in vortex-lines, while the term $\Delta\phi$ would be taken to satisfy the general irrotational motion.

5. To investigate the energy within a surface drawn within a fluid we get, by continually using a modification of Green's Theorem and omitting Thomson's correction,

$$-2T=\int\sigma^2 dv=\int S(\Delta\phi+k\nabla\psi)\sigma dv=\int S\sigma\nabla\phi dv+\int Sk\sigma\nabla\psi dv$$
$$=-\int S\nabla\sigma\phi dv+\int S\sigma\phi\nu dS-\int S\nabla k\sigma\psi dv+\int Sk\psi\sigma\nu dS$$
$$=-\int S\nabla\sigma(\phi+k\psi)dv-\int S\psi\nabla k\sigma dv+\int S(\phi+k\psi)\sigma\nu dS$$
$$=-\int S\psi\nabla k\sigma dv+\int S(\phi+k\psi)\sigma\nu dS\,; \quad . \quad . \quad . \quad . \quad . \quad \text{(V.)}$$

where ν stands for the unit normal to the surface, and where the last term vanishes if there is no flow across the bounding surface.

We may then use the equation of continuity to give other forms to the volume integral. Thus

$$S\psi\nabla k\sigma=\psi S\nabla k(\nabla\phi+k\nabla\psi)$$
$$=\psi S(\nabla k\nabla\phi-k\nabla^2\phi-k^2\nabla^2\psi),$$

forms which will allow us to use Green's theorem again.

From the rate of change of the circulation in a closed circuit moving with the fluid we get

$D_t\int S\sigma d\tau=0$, where $d\tau$ is an element of the circuit, or

$$\int SD_t\sigma d\tau+\int S\sigma d\,.\,D_t\tau=\int SD_t\sigma d\tau+\int S\sigma d\sigma=0.$$

The second is zero over every closed circuit. In order that the first may be so we must have $V\nabla\,.\,D_t\sigma=0$, or referring to

$$V\{\nabla D_t k\,.\,\nabla\psi+\nabla k\nabla\,.\,D_t\psi\}=0, \quad . \quad . \quad . \quad \text{(VI.)}$$

a further justification for our assumption.

The simplest way of finding $D_t\rho$ is as follows:—We have seen that

$$D_t\sigma=\dot\sigma-(S\,\nabla)\,\sigma=\dot\sigma-2V\sigma\rho-\tfrac{1}{2}\nabla\sigma^2.$$

Operate upon this with $V\nabla$, then

$$V\nabla D_t\sigma=\dot{\rho}-2V\nabla V\sigma\rho=\dot{\rho}-(S\sigma\nabla)\rho+(S\rho\nabla)\sigma$$
$$=D_t\rho+(S\rho\nabla)\sigma.$$

But, from the circulation, $V\nabla D_t\sigma=0$;

$$\therefore D_t\rho=-(S\rho\nabla)\sigma. \quad . \quad . \quad . \quad . \quad . \quad . \quad \text{(VII.)}$$

The value in terms of k and ψ is not so simple as to deserve notice.

The geometry of the motion is not easily explained, owing to the fact that ϕ is not the third surface satisfying $D_t(x)=0$; also k and ψ will not generally be independent, as the condition for irrotational motion is that they should touch at the points of intersection.

XX. *Notes on some Quaternion Transformations.* By R. F. Gwyther, M.A.

Read February 24th, 1880.

The following theorems are frequently required in physical problems, especially in the motion of fluids.

I.

If τ denote the vector of any point and ∇ Hamilton's operator, and if ρ and σ are any vector functions of τ, then

$$\nabla(\rho\sigma)=\nabla\rho\sigma-\rho\nabla\sigma+2(S\rho\nabla)\sigma. \quad . \quad . \quad . \quad . \quad \text{(I.)}$$

More generally, if ρ and σ be any vectors depending on the scalars a, b, c, &c., and if α, β, γ, &c. be any vectors

whatever, and if $\alpha d_a+\beta d_b+\gamma d_c+$ &c. $=\mathrm{D}$ (where d_a denotes differentiation with regard to a, &c.), then

$$\mathrm{D}(\rho\sigma)=\mathrm{D}\rho\,.\,\sigma-\rho\mathrm{D}\sigma+2(\mathrm{S}\rho\mathrm{D})\sigma.$$

For

$$\alpha d_a(\rho\sigma)=\alpha d_a\rho\,.\,\sigma+\alpha\rho\,.\,d_a\sigma$$

$$=\alpha d_a\rho\,.\,\sigma-\rho\alpha\,.\,d_a\sigma+2(\mathrm{S}\rho\alpha d_a)\sigma,$$

since

$$\alpha\rho+\rho\alpha=2\mathrm{S}\rho\alpha.$$

If we now form the similar quantities for βd_b, &c., and add the respective sides of the equation thus formed,

$$\therefore\ \mathrm{D}(\rho\sigma)=\mathrm{D}\rho\,.\,\sigma-\rho\mathrm{D}\sigma+2(\mathrm{S}\rho\mathrm{D})\sigma.\quad .\quad .\quad .\ \text{(II.)}$$

But τ may be written $a\alpha+b\beta+c\gamma$, where α, β, γ are rectangular unit vectors. D in this case becomes identical with ∇, and the preceding form may be deduced.

The more general form is occasionally useful.

From I. we may, by taking scalar and vector parts, get

$$\mathrm{S}\nabla(\rho\sigma)=\mathrm{S}\,.\,\nabla\rho\sigma-\mathrm{S}\,.\,\rho\nabla\sigma,\quad .\quad .\quad .\quad .\ \text{(III.)}$$

$$\mathrm{V}\nabla(\rho\sigma)=\mathrm{V}\nabla\rho\sigma-\mathrm{V}\rho\nabla\sigma+2(\mathrm{S}\rho\nabla)\sigma,\quad .\quad .\ \text{(IV.)}$$

whence we may deduce by putting $\rho=\sigma$

$$\mathrm{V}\nabla\sigma^2=2\mathrm{V}\,.\,\mathrm{V}\nabla\sigma\,.\,\sigma+2(\mathrm{S}\sigma\nabla)\sigma.\quad .\quad .\ \text{(V.)}$$

We may also deduce from (1) expressions for $\nabla(\mathrm{V}\rho\sigma)$ and $\nabla(\mathrm{S}\rho\sigma)$.

Thus $\nabla(\mathrm{V}\rho\sigma)=\frac{1}{2}\nabla(\rho\sigma-\sigma\rho)$

$$=\mathrm{S}\,.\,\nabla\rho\sigma-\mathrm{S}\,.\,\rho\nabla\sigma+(\mathrm{S}\rho\nabla)\sigma-(\mathrm{S}\sigma\nabla)\rho+\mathrm{S}\nabla\rho\,.\,\sigma-\mathrm{S}\nabla\sigma\,.\,\rho,$$

and

$$\nabla(\mathrm{S}\rho\sigma)=\mathrm{V}\,.\,\mathrm{V}\nabla\rho\,.\,\sigma-\mathrm{V}\rho\mathrm{V}\nabla\sigma+(\mathrm{S}\rho\nabla)\sigma+(\mathrm{S}\sigma\nabla)\rho.$$

These forms simplify still further when in fluid-motion σ is the velocity at any point, and $V\nabla\sigma = 2\rho$ gives the rotation at that point, in which case $S\nabla\rho = 0$.

We have then

$$V\nabla(V\rho\sigma) = (S\rho\nabla)\sigma - (S\sigma\nabla)\rho - S\nabla\sigma\,.\,\rho,$$

$$S\nabla(V\rho\sigma) = S\,.\,\nabla\rho\sigma - \tfrac{1}{2}\rho^2,$$

and

$$\nabla(S\rho\sigma) = V\nabla\rho\sigma + (S\rho\nabla)\sigma + (S\sigma\nabla)\rho.$$

Again, Helmholtz's notation for the form of σ gives $\sigma = \nabla(\phi + \omega)$, where ϕ is a scalar and $S\nabla\omega = 0$; and these formulæ are applicable in the reduction of the equations.

A slight adaptation of this method enables us to prove that, if p and q are quaternion functions of τ, we should have

$$\nabla(pq) = \nabla pq + Kp\nabla q + 2(Sp\nabla)q. \quad . \quad . \text{ (VI.)}$$

II.

These results are very useful in obtaining modified forms of Green's theorem.

The general form of the theorem is

$$\int S\nabla\psi dv = \int S\psi\nu\,.\,ds,$$

where ψ is a single-valued vector function in simply connected space, dv an element of volume, ds of its bounding surface, and ν the unit vector normal to ds drawn outwards.

Case I. Let $\psi = V(\rho\sigma)$.

Then by (V)

$$S\nabla\psi = S\nabla\rho\sigma - S\rho\nabla\sigma,$$

and we get

$$\int S\nabla\rho\sigma dv - \int S\rho\nabla\sigma dv = \int SV\rho\sigma\,.\,\nu ds = \int S\rho\sigma\nu ds. \quad . \text{ (VII.)}$$

CASE II. Let $\psi = \phi\sigma$, where ϕ is a scalar.
Then

$$\nabla\psi = \nabla\phi \cdot \sigma + \phi\nabla\sigma,$$

and

$$\int S\nabla\phi\sigma \,.\, dv + \int S\nabla\sigma \,.\, \phi dv = \int \phi S\sigma v ds. \quad . \quad . \quad . \quad \text{(VIII.)}$$

Stokes's theorem can be made by similar treatment to give varied forms, both more and less simple.

XXI. *Colorimetry.*—Part IV.

By JAMES BOTTOMLEY, D.Sc.

Read before the Physical and Mathematical Section, April 13th, 1880.

On the Colour-relations of Nickel and Cobalt.

FOR some experiments which I was making in colorimetry I wished to obtain a solution which would absorb all the kinds of light in the same ratio, so that whatever sort of light we started with, after penetration through such a solution, it would remain the same in character, the only variation being a change in intensity. Hence through such solutions white surfaces would appear grey of various shades, verging towards blackness as the length of the column increased. Such a fluid we might call a soluble black. I am not aware of any single fluid that fulfils the above conditions. It might be said, Why not use ink? but such specimens of ink as I have examined are bluish or violet on copious dilution. Moreover the colour alters

with the degree of oxidation; also it seems to be colouring-matter in suspension rather than in solution. I had some hopes of succeeding by mixing solutions of nickel and cobalt salts. On reference to the 'Philosophical Magazine,' vol. vi. p. 15, I find that the colour-relations of nickel and cobalt had been studied by Mr. Thomas Bayley with a view to the quantitative determination of these metals founded upon the complementary character of their colours. He states, "The fact will have been observed by chemists that solutions of nickel and cobalt salts are so far complementary in colour that when they are mixed together the resulting liquid, if moderately dilute, is hardly to be distinguished from pure water." After considering the nature of the absorption-spectra of nickel and cobalt salts, he states, "If the spectra were exactly complementary, on superimposing the nickel spectrum upon the cobalt spectrum the dark part on the one would cover exactly the light part on the other. This, however, though nearly the case, is not exactly so......this is why the solution obtained by mixing strong solutions of nickel and cobalt is not grey, but reddish brown in colour." Some experiments which I made seemed to confirm the opinion of Mr. Bayley. The nickel solutions contained 0·05 grm. of $NiSO_4$ per cub. c.; and the cobalt solution contained 0·05 grm. $CoSO_4$ per cub. c. A mixture consisting of 50 cub. c. of cobalt solution with 100 cub. c. of nickel solution, contained in a white porcelain basin, seemed to be a grey tinted with pink in the shallower parts, and having the tendency to pass into a yellowish tint as the depth increased. I now poured the fluid into a tall glass cylinder covered externally with black cloth except a circular aperture of 8 millim. diameter at the bottom. When I looked through the column of fluid at a white surface, the colour was decided, resembling somewhat the pigment known as yellow ochre.

Also with a less proportion of cobalt to nickel, namely 20 cub. c. of cobalt solution to 50 cub. c. of nickel solution, I still obtained a tint in which yellow seemed to predominate. Had I employed solutions so dilute that no colour was perceptible in the mixture, this would not strictly imply that the colours were complementary, but that the resulting tint was too feeble to produce the impression of colour; and if we filled a long tube with such a dilute solution the colour might again become manifest. Moreover my aim was not to mix two coloured solutions so as to obtain a fluid which exercised no perceptible absorption of light, but to obtain a fluid which would exercise a considerable absorption subject to a certain condition. The following consideration seemed to me to render it hopeless to obtain a soluble black by nickel and cobalt only. A solution of cobalt when dilute is pink; but if we look through a considerable thickness or through a concentrated solution, the pink shows a tendency to pass into a scarlet. This shows that as the quantity of the salt increases, the ratio of the yellow to the red increases. The colour of the undissolved salt is brownish red; and the colour of the solution seems to approximate towards this as the concentration increases. Hence the colour of a solution of cobalt alters not only in intensity, but also in kind, as the amount of the salt is increased. On the other hand, the green of a solution of nickel varies in intensity, but does not seem to vary in character, at least in any marked manner, as the quantity of the salt increases. In order that it should be generally complementary in character to cobalt, any inconstancy in the ratio of the red to the yellow of the latter would require a corresponding variation in the ratio of the yellow to the blue in the former, and the tint ought to pass from an emerald-green to a bluish green. As this does not seem to be the case, it would follow that even if

we mixed nickel and cobalt so as to obtain a perfect grey for a column of a definite length, a column longer or shorter than this would still retain some colour. My experiments seemed to indicate a deficiency of blue in the mixture; and this I thought might be supplemented by another salt. So I tried the addition of sulphate of copper. After some trials I got a solution containing in 1000 cub. c. 7·275 grms. $NiSO_4$, 4·868 grms $CoSO_4$, and 11·468 $CuSO_4$: the solutions also contained 30 cub. c. of strong sulphuric acid; this I added to guard against any possible formation of subsalts on copious dilution. This solution seemed nearer to what I wanted than a solution of nickel and cobalt only. It did not, however, appear wholly free from colour; and possibly a variation of the quantities might have given a better result; also the tint seemed to vary somewhat with the nature and intensity of the incident light. When in the failing light of approaching evening I held the containing bottle against the grey sky, I thought that there remained a somewhat pinkish tint, whilst in the colorimeter, when looking at an external white surface through a column sufficiently long to produce a perceptible absorption, I thought the solution had a bluish tint. When viewed against gaslight, it gave a greenish tint. Within the range of coloured fluids in chemistry there may be some which, if combined, would yield a mixture absorbing all colours in the same ratio, so as to be truly a soluble black. The preparation of such a fluid would be an interesting problem in physics. It seems to me that we might also have such fluids which, on spectral analysis, would show not an absorption of all colours in the same ratio, but would be resolved into a violet and yellow, or an orange and blue, or red and green, or some other combination of colours of a complementary character.

Remarks on the Formulæ for the Intensity of Light that has passed through absorbing media, and on a Method of Experimental Verification.

In my last paper on colorimetry I pointed out that the function which expresses the connexion of the intensity of light with the quantity of colouring-matter is of the same form as the function expressing the relationship of the intensity and the length of the absorbing column; and if we accept Herschel's formula $\Sigma a k^t$ for the latter relationship, then an expression of the form $\Sigma a \kappa^Q$ must be taken to express the former relationship. The connexion of these two may be shown more directly than I indicated in my last paper. If we grant one of the laws, the other may be deduced from it as a corollary. Take, for instance, the law as given by Herschel,

$$T = a_1 k_1{}^t + a_2 k_2{}^t + \text{\&c.}$$

Now it is manifest that, if q be the quantity of colouring-matter per unit of length, we may write the above formula

$$T = a_1 k_1{}^{q\frac{t}{q}} + a_2 k_2{}^{q\frac{t}{q}} + \text{\&c.}$$

For $k_1{}^{\frac{1}{q}}$, $k_2{}^{\frac{1}{q}}$, $k_3{}^{\frac{1}{q}}$, &c. substitute new constants κ_1, κ_2, κ_3, &c. Then we may write

$$T = \Sigma a \kappa^{qt}.$$

Since q denotes the quantity of colouring-matter per unit of length and t the total length, we shall have

$$Q = qt,$$

where Q denotes the whole quantity of colouring-matter; so that we finally deduce

$$T=\Sigma a\kappa^{Q}.$$

As the basis of a method of colorimetry, I took the relationship that the length of the column was inversely as the quantity of colouring-matter present when the colour was made constant. It may be readily shown to be a consequence of the laws stated above. Suppose C to be the constant colour, then

$$C=\Sigma ak^{qt}.$$

The form of the equation shows that C is the sum of a number of constants C_1, C_2, C_3, &c., such that

$$C_1=a_1k_1^{qt},$$

$$C_2=a_2k_2^{qt},$$

$$\ldots=\ldots$$

$$C_n=a_nk_n^{qt};$$

whence we obtain

$$\log\frac{C_1}{a_1}=qt\log k_1,$$

$$\log\frac{C_2}{a_2}=qt\log k_2,$$

$$\ldots\ldots=\ldots\ldots$$

$$\log\frac{C_n}{a_n}=qt\log k_n;$$

and by addition we obtain

$$\log\frac{C_1}{a_1}+\log\frac{C_2}{a}+\&c.=qt(\log k_1+\log k_2+\&c.),$$

$$\text{or } qt=\frac{\log\left(\dfrac{C_1C_2}{a_1a_2}\cdots\cdots\dfrac{C_n}{a_n}\right)}{\log(k_1k_2\ldots\ldots k_n)}=\text{constant.}$$

In my last paper I stated that the law of absorption of light given by Herschel appears to have been obtained *a priori*; I have not found in his memoirs any experimental confirmation of it. The form of the expression has a somewhat formidable appearance, inasmuch as it involves the measurements of infinite varieties of light. But suppose that in the formula Σak^t, k is the same for every species of light; then we may write $T=k^t\Sigma a$ or $T=k^tI$, if I denote the incident light. In such a case the emergent light will be of the same nature as the incident light, and will differ only in intensity. Suppose the incident light to be white, the emergent light will be a white of less intensity—that is, will be a grey approaching to blackness as the length of the column increases. A fluid medium affecting white light in this way we might call a soluble black; and my aim in seeking to obtain such a fluid was to apply it to the confirmation of the law. In a previous note I stated that I had tried to obtain such a body. What I got was not wholly satisfactory; but I thought that with it I might obtain some approximate results. The solution I used consisted of 500 cub. c. of the previously mentioned fluid with 500 cub. c. of distilled water.

The mode in which I proposed to operate was as follows. Take two white lights of different intensities, say W_1 and W_2, and look at them through the liquid. Suppose the lengths of the columns when equality of intensity is obtained to be t_1 and t_2, then

$$W_1k^{t_1}=W_2k^{t_2}.$$

Suppose we alter the lengths of the columns, and in a second experiment we find

$$W_1 k^{t'_1} = W_2 k^{t'_2};$$

by cross multiplication and elimination of the common factor $W_1 W_2$ we obtain

$$k^{t_1} k^{t'_2} = k^{t_2} k^{t'_1},$$

or, as we may write it,

$$k^{t_1 + t'_2} = k^{t_2 + t'_1}.$$

Then taking the logarithms of both sides and dividing by $\log k$ we get finally

$$t_1 + t'_2 = t_2 + t'_1.$$

In this way I proposed to test the law.

I took as standard tints a smooth surface of $BaSO_4$ and another of $BaSO_4$, and carbon in the proportion of 10 grms. of $BaSO_4$ to 0·006 grm. of carbon; these materials were intimately mixed, and the powder reduced by pressure to a flat surface. The colorimeters used were glass cylinders covered externally with black cloth, the circular apertures at the bottom admitting light being 8 mm. in diameter.

One experiment gave the following results. Length of column 22·2 c., standard tint W_1 ($BaSO_4$). I now attempted to get the same intensity when looking through the second cylinder at tint W_2 ($BaSO_4$ + carbon). The mean of two trials gave 14·8 c. as the equivalent column. Hence we have the following results:—

$$W_1 k^{22 \cdot 2} = W_2 k^{14 \cdot 8}.$$

I now made the length of the column over W_1 13 centim. The equivalent column over W_2 was, as the mean of two trials, 7·5. Hence

$$W_1 k^{13} = W_2 k^{7 \cdot 5}.$$

From these experiments we get as the sum of t_1 and t'_2 29·7, and the sum of t'_1 and t_2 27·8.

A second experiment gave

$$W_1 k^{13} = W_2 k^{6 \cdot 55},$$

$$W_1 k^{22 \cdot 2} = W_2 k^{15 \cdot 45}.$$

Here $t_1 + t'_2 = 28 \cdot 45$, and $t_2 + t'_1 = 28 \cdot 76$.

A third experiment gave

$$W_1 k^{16 \cdot 8} = W_2 k^{10 \cdot 65},$$

$$W_1 k^{14 \cdot 3} = W_2 k^{8 \cdot 5}.$$

Here $t_1 + t'_2 = 25 \cdot 3$, and $t'_1 + t_2 = 24 \cdot 93$.

A fourth experiment gave the following results :—

$$W_1 k^{22 \cdot 2} = W_2 k^{14 \cdot 9};$$

$$W_1 k^{12 \cdot 5} = W_2 k^{6 \cdot 2}.$$

Here $t_1 + t'_2 = 28 \cdot 4$, and $t'_1 + t_2 = 27 \cdot 4$.

A fifth experiment gave

$$W_1 k^{25 \cdot 1} = W_2 k^{16 \cdot 65},$$

$$W_1 k^{13 \cdot 7} = W_2 k^{6 \cdot 85}.$$

Here we have $t_1 + t'_2 = 31 \cdot 95$, and $t'_1 + t_2 = 30 \cdot 35$.

It seems to me that the above results are as favourable

as might be expected, considering the difficulties of the inquiry; and even if all external circumstances necessary for the successful completion of such experiments had existed, there would yet remain the difficulty of deciding about the equality of two grey tints. In such matters it is difficult to say where judgment ends and fancy begins.

That any discrepancies might be due to such a cause was shown by the following experiments:—I took the two cylinders and poured from one into the other with the intention of obtaining the same tint in both. In one trial I could not very clearly distinguish a column 16·4 centim. long from one 14·6 centim. long; and in a second trial a column 16·2 centim. long seemed to give the same tint as a column 14·6 centim. long. In the above experiments I used one eye only, namely the right one.

I also made the following experiments:—I took as the standard of intensity $\dot{W}$ seen through a column 12·5 centim. long. On a former occasion I had made 6·2 as the equivalent column to be used with W_2. On the present occasion I thought 6·5 centim. gave a nearer result; so I took the column at this length. Now, if the law of absorption of light be true, if we increase both columns by the same quantity, the intensities should again correspond. So I added 4 centim. to each, making one column 16·5 and the other 10·2. I thought that the tints were the same. I now made one column 20·5 and the other 14·2. Again I thought the tints were the same. Finally I made one column 24·5 and the other 18·2. The tints seemed again to correspond.

XXII. *Colorimetry.*—Part V. *On the Absorption of Light by Turbid Solutions.* By James Bottomley, D.Sc.

Read before the Physical and Mathematical Section, April 27th, 1880.

Media containing colouring-matter may be divided into two classes, transparent and turbid. It might be considered that in their behaviour with regard to light they were wholly dissimilar. But the question arises, Is not the difference between transparency and turbidity one of degree? Experience in the laboratory brings under our notice cases where matter is so finely divided as not to be separable from fluids by filtration, and showing but slight tendency to settle as a precipitate; and in some cases we have liquids which are apparently transparent, and yet are considered to hold solid matter in suspension. May not such examples be intermediate between solutions and cases where extremely fine particles are uniformly diffused through some transparent medium? If, then, we consider the passage from transparency to turbidity a continuous one, and if for a transparent fluid we have established some law of absorption of light, may not the same law be applicable to a turbid solution? The subject seemed to me interesting both as a scientific inquiry and on account of its application to quantitative analysis.

Suppose we have diffused through a liquid some finely divided solid matter; the action of such a turbid solution on light will be twofold: it disperses light and it absorbs light. By reason of the first action we are made aware of the colour of the turbidity; by reason of the second any

object seen through the liquid seems of diminished distinctness.

As a typical case take carbon diffused through water. In a paper which I read at the last Meeting of the Physical and Mathematical Section I alluded to some attempts to obtain a soluble black in order to make some experiments on the absorption of light. To assist my judgment as to the appearance which such a liquid should present, I had a cylinder containing a little carbon diffused through water. Weak diffusions of carbon in the colorimeter gave the same appearance when I looked at external white surfaces as I should have expected a liquid containing a soluble black in solution to give under the same conditions. A diffusion of carbon both disperses light and absorbs light. The dispersion gives rise to a greyish tint, due to the light coming from the innumerable particles of carbon. Owing to the absorption a white surface seen through the column of liquid seems of diminished whiteness. Hence, under ordinary circumstances the light which comes to the eye has a twofold origin, part being transmitted light and part dispersed light. My first object was to dissociate these two phenomena. I therefore used cylinders which were covered with black cloth, admitting light by circular apertures at the bottom, 8 millim. in diameter. In this way the dispersed light was almost wholly cut off. In some cases a feebly nebulous light could be seen around the apertures; but it was very slight and did not interfere with experiments to determine the absorptive properties. With any attempt to explain by physical optics the analogy of the absorption of light by solutions and diffusions I have nothing to do; but, on the supposition that there was continuity, I was led to expect that a function of the same form would express the intensity of the transmitted light in both cases. Also, independently of such considerations, it

seemed probable, from reflection upon the mode of distribution of the particles throughout the mass of fluid, that the function would be the same as for transparent solutions. In the latter case both experiment and reasoning from first principles concur in giving a formula Σak^t for the intensity of light passing through a column t units long. As I have pointed out in another paper, this implies an expression of the form $\Sigma a\kappa^Q$, denoting the connexion between the transmitted light and the quantity of colouring-matter. Therefore we might expect a similar expression to hold in the case of diffusions (the term seems to me more convenient than to speak of turbid solutions). If such be the case, one consequence will be that, if we have two cylinders containing in equal bulks of water Q and Q′ of solid matter in suspension, if we adjust the columns so as to obtain the same intensity of light when we regard an external white surface, then the lengths of the columns will fulfil the condition $Qt = Q't'$.

The carbon that I used was lamp-black recalcined in a covered platinum crucible. After being so treated it seemed blacker than it did before. Of this 0·417 gram was ground up in a mortar with 10 drops of a solution of gum. The contents of the mortar were then rinsed out with water and diffused by shaking through 500 cub. c. of water. This was poured into a cylinder; the length of the column was 22·5 centimetres. After the lapse of 24 hours I drew off by a pipette the upper portion; the column so removed was 15·8 centim. long. This carbon diffusion contained so much solid matter that I found it inconveniently strong for experiments; so, as occasion required, I made weaker diffusions, containing in 250 cub. c. 5, 10, 20, or 40 of the stronger diffusion. Of these weaker liquids portions were taken and mixed with water so as to yield a bulk of 500 cub. c. I stated above that

the carbon was ground up with 10 drops of gum, so as to yield a smooth, thick consistence. The amount of dry gum would be very small; nevertheless it had a remarkable effect in increasing the adhesion of the carbon to the water. When I had shaken up carbon alone with water it had sensibly subsided after the lapse of a few hours. After the addition of so small a quantity of gum the tendency to deposit is much diminished. The following are the details of some experiments. The strength of the carbon diffusion is given in terms of the number of cub. c. of the strong carbon diffusion in 500; cub. c. of water. The external white surface was a sheet of white paper. In all cases I have used the right eye only. The number under A de-denotes the mean of two trials got by pouring into the cylinder, and therefore likely to yield too low results. B denotes the mean of two trials got by pouring out of the cylinder, and therefore likely to give too high results. C denotes the mean of A and B. D denotes the length required by theory.

Standard-diffusion 1·2 cub. c. in 500 cub. c. of water, length of column 21·2.

Exp. I.—Comparison-diffusion contains 1·8 cub. c. in 500 cub. c.

A	B	C	D
14·35	16·35	15·35	14·13

A second trial gave

A	B	C	D
12·65	14·65	13·65	14·13

Exp. II.—Comparison-diffusion contains 2·4 cub. c. in 500 cub. c.

A	B	C	D
10·6	11·25	10·93	10·6

Exp. III.—Comparison-diffusion contains 3·0 cub. c. in 500 cub. c.

A	B	C	D
8·4	9·25	8·83	8·48

Exp. IV.—Comparison-diffusion contains 3·6 cub. c. in 500 cub. c.

A	B	C	D
6·85	7·6	7·22	7·07

Exp. V.—Comparison-diffusion contains 4·2 cub. c. in 500 cub. c.

A	B	C	D
5·6	6·1	5·85	6·06

Exp. VI.—Comparison-diffusion contains 4·8 cub. c. in 500 cub. c.

A	B	C	D
5·1	5·55	5·32	5·3

Exp. VII.—Comparison-diffusion contains 5·4 cub. c. in 500 cub. c.

A	B	C	D
4·1	4·5	4·3	4·71

On another occasion I got a nearer result, as follows:—

A	B	C	D
4·7	4·95	4·82	4·71

Exp. VIII.—Comparison-diffusion contains 6 cub. c. in 500 cub. c.

A	B	C	D
4·13	4·58	4·36	4·24

Exp. IX.—Comparison-diffusion contains 6·6 cub. c. in 500 cub. c.

A	B	C	D
3·98	4·25	4·11	3·85

Exp. X.—Comparison-diffusion contains 7·2 cub. c. in 500 cub. c.

A	B	C	D
3·48	3·75	3·61	3·53

Exp. XI.—Comparison-diffusion contains 7·8 cub. c. in 500 cub. c.

A	B	C	D
3·23	3·45	3·34	3·26

Exp. XII.—Comparison-diffusion contains 8·4 cub. c. in 500 cub. c.

A	B	C	D
3·03	3·45	3·24	3·03

Exp. XIII.—Comparison-diffusion contains 9 cub. c. in 500 cub. c.

A	B	C	D
2·9	3	2·95	2·82

Exp. XIV.—Comparison-diffusion contains 9·6 cub. c. in 500 cub. c.

A	B	C	D
2·83	3·05	2·94	2·65

Exp. XV.—Comparison-diffusion contains 14·4 cub. c. in 500 cub. c.

A	B	C	D
1·98	2·08	2·03	1·77

Exp. XVI.--Comparison-diffusion contains 19·2 cub. c. in 500 cub. c.

A	B	C	D
1·6	1·58	1·59	1·32

Many of the above numbers are close approximations. I also tried columns of the lengths given by theory in all experiments, except in the ninth, where by an oversight I neglected to do so. The tints so obtained were in every case satisfactory until I reached Experiment XIV. In this and in the succeeding experiments I thought that columns of the theoretical lengths gave tints slightly lighter than the standard diffusion. On some occasions I have wavered in my opinion that the theoretical column gave too light a tint in the fourteenth experiment. At another time I got the following result :—

Exp. XVII.—Comparison-diffusion contains 9·6 cub. c. in 500 cub. c.

A	B	C	D
2·8	2·95	2·87	2·65

Here again the number under C is a little greater than that under D. I afterwards repeated the experiment with this variation—that the standard diffusion was contained in the comparison-cylinder and the comparison-diffusion in the standard cylinder.

Exp. XVIII.—Standard diffusion contains 1·2 cub. c. in 500 cub. c.; length of column 22·6.

Comparison-diffusion contains 9·6 cub. c. in 500 cub. c.

A	B	C	D
2·85	3·07	2·96	2·82

On another occasion I got :—

A	B	C	D
2·85	3·15	3·0	2·82

Here again there still remains a tendency to make the column a little longer than the theoretical.

I repeated the experiments in which the strength of the comparison-diffusion is several times a multiple of the strength of the standard diffusion. The strength of the standard diffusion was the same as in the previous experiments; and the length of the column was 21·2.

Exp. XIX.—Comparison-diffusion contains 9·6 cub. c. in 500 cub. c.

A	B	C	D
2·63	2·98	2·8	2·65

When I actually tried the theoretical column I thought that the tint was, perhaps, slightly lighter.

Exp. XX.—Comparison-diffusion contains 14·4 cub. c. in 500 cub. c.

A	B	C	D
1·9	2·15	2·02	1·77

The tint given by the theoretical column I thought slightly lighter.

Exp. XXI.—Comparison-diffusion contains 19·2 cub. c. in 500 cub. c.

A	B	C	D
1·55	1·63	1·59	1·32

The tint given by the theoretical column I thought slightly lighter.

Exp. XXII.—Comparison-diffusion contains 24·0 cub. c. in 500 cub. c.

A	B	C	D
1·15	1·3	1·22	1·06

The tint given by the theoretical column I thought slightly lighter, but hardly distinguishable.

Exp. XXIII.—Comparison-diffusion contains 28·8 cub. c. in 500 cub. c.

A	B	C	D
1·0	1·1	1·05	0·88

The tint given by the theoretical column I thought slightly lighter.

Exp. XXIV.—Comparison-diffusion contains 33·6 cub. c. in 500 cub. c.

A	B	C	D
0·88	0·9	0·89	0·76

The tint given by the theoretical column I thought slightly lighter.

Exp. XXV.—Comparison-diffusion contains 38·4 cub. c. in 500 cub. c.

A	B	C	D
0·73	0·83	0·78	0·66

The tint given by the theoretical column I thought slightly lighter; they were, however, very nearly the same.

This second series of experiments confirms the result of the first series, that there is a slight departure from the rule when the strength of one diffusion is several times a multiple of the other. Both series show a tendency to make the column in the comparison-cylinder slightly too long. We must not hastily conclude that in these cases the theory is not applicable. It seems to me that we should expect such a result; for the medium is not perfectly transparent, and in one cylinder we have a column of this medium (water) several times a multiple of the length of the column in the other. This would require some slight compensation. In these experiments I have taken the lower level of the meniscus as the proper reading. In some of the stronger

diffusions it was a little difficult to determine this exactly. On the whole I think that the above experiments are favourable to the assumption that for a column of fluid containing finely divided carbon in suspension the relationship $Qt=$ constant holds, if the intensity of the transmitted light remain constant. If we represent the above results graphically, and take as the theoretical curve the rectangular hyperbola $xy=25\cdot44$, it will be seen that the results of the experiments do not depart far from the curve.

In a paper read at the last Meeting of the Section I suggested a method for testing the assumed laws of the absorption of light. I have also applied the same reasoning and method of experiment to carbon diffusions. In one series of experiments I took a diffusion containing 1·934 cub. c. in 500 cub. c. The standard shades of grey used were:—one consisting of 10 grms. $BaSO_4$ and 0·012 of lamp-black (this I denote by Wa); the other consisted of 10 grms. of $BaSO_4$ and 0·048 of lamp-black (this I denote by Wb). The materials were well incorporated by shaking and grinding. To the powder I then added a little water, so as to obtain a mixture of suitable consistence to be used as a paint. This was applied by a brush to pieces of cardboard, so as to obtain uniform surfaces. These surfaces were then dried. The colorimeters employed were the same as in the last experiments. I looked at Wb through a column 3 cm. long. I held the other cylinder over Wa, and endeavoured to get the same tint. The mean of two columns, one probably a little too long and the other probably a little too short, gave 8·2 as the proper length; so I made the column of this length; I thought that the tints were the same. Now, if the law hold with regard to turbid solutions, if we increase both columns by the same length the tints will again correspond. I took 4 cm. as the common increment; the lengths of the columns were made 7 and 12·2; I thought the

resulting tints equal. The lengths of the columns were made 11 and 16·2; I thought they were about equal, possibly Wa slightly lighter. My impression varied a little with the illumination of the surfaces. The lengths of the columns were now made 15 and 20·2; the tints seemed about equal, possibly Wa slightly lighter. The difference, if any, must be small, as is shown by the following experiment. Taking Wb seen through a column 15 cm. long as the standard tint, I endeavoured to get the same tint with Wa seen through the other cylinder. For the lower limit I got 19·9 cm., and for the upper limit 21 cm.; the mean of these is 20·45, not far removed from 20·2. Afterwards I thought a column of this length satisfied. I now made the lengths of the colmmns 19 and 24·2; I thought that the tints again corresponded.

Some experiments of a similar nature were made with a stronger diffusion, it contained 3·747 cub. c. in 500 cub. c. The tints of grey used were the same as in the last experiments. The standard tint at the commencement was Wb seen through a column 4 cm. long. To get a similar tint with Wa one determination for the upper limit gave 6·5, and one determination for the lower limit gave 6·1. The mean of these is 6·3. A column of this length seemed to satisfy. The common increment is 3 cm. The columns were made 7 and 9·3 cm. long; the tints seemed to correspond. The columns were made 10 and 12·3 cm. long; the tints again seemed to correspond. The columns were made 13 and 15·3 cm. long; the tints seemed again to correspond. Finally the columns were made 16 and 18·3 cm. long. The tints again corresponded. In the last two experiments the greys obtained were very deep. I also made the following experiment:—I took two grey tints: one consisted of 10 grms. $BaSO_4$ and 0·042 grm. of lamp-black; the other consisted of 10

grms. $BaSO_4$ and 0·4003 grm. of lamp-black. These greys were made into a paint by the addition of a little water, and pieces of cardboard covered with them. They were then dried. These we may denote by W_α and W_β. I looked at W_β through a column 4·1 cm. long, and endeavoured to get a similar tint with W_α under the other cylinder. For the upper limit the column was 6·95 cm. long, and for the lower limit 6·2 cm. long. The mean is 6·57 cm. I next altered the length of the column over W_β to 11·75. In the other cylinder for the upper limit the column was 14·9 cm., and for the lower limit 14·8; the mean is 14·85. Hence we have

$$W_\beta K^{4\cdot1} = W_\alpha K^{6\cdot57},$$

$$W_\beta K^{11\cdot75} = W_\alpha K^{14\cdot85}.$$

By cross multiplication and elimination of W_α, W_β,

$$K^{4\cdot1} K^{14\cdot85} = K^{6\cdot57} K^{11\cdot75}.$$

Theory requires the sums of the indices to be equal.

The sum on the right hand is 18·32, and on the left 18·95. The difference is, I think, not greater than what might be due to errors of observation.

XXIII. *On the Conditions of the Motion of a Portion of Fluid in the Manner of a Rigid Body.* By R. F. Gwyther, M.A.

Read November 2nd, 1880.

The condition that a portion of fluid may comport itself as a rigid body, or that fluid may remain in a state of relative rest upon or within a moving solid, has not, as far as I am aware, been mathematically investigated. We know, however, that in certain cases, as on the surface of the earth, the condition can be realized, or that any deviation has not been discovered by undirected investigation.

In the case considered the velocity at points in the fluid must consist of a common linear velocity and a common angular velocity about some axis, moving or fixed. Therefore, using the quaternion notation, the velocity must be of the form

$$\sigma = \Sigma + V\epsilon\tau,$$

where Σ is the common linear velocity, ϵ the vector-axis of instantaneous rotation, and τ the vector of any point in the fluid.

The equation of motion is

$$D_t\sigma + \frac{1}{\varrho}\nabla p = \alpha, \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

α denoting the force acting on the element of the fluid, p and ϱ having the usual meanings. Under the condition stipulated no force due to viscosity is called into action.

If ϱ be a function of p only, we may write

$$\frac{1}{\varrho}\nabla p = \nabla P.$$

Substituting the required form of σ, we get

$$\dot{\Sigma}+\mathrm{V}\dot{\epsilon}\tau+2\mathrm{V}\epsilon(\Sigma+\mathrm{V}\epsilon\tau)=a-\nabla\mathrm{P}.. \quad . \quad . \qquad (2)$$

Now act upon this with ∇ (which will not affect either Σ or ϵ), and afterwards take the vector and scalar parts, thus

$$\nabla(\mathrm{V}\dot{\epsilon}\tau+2\epsilon^2\tau-2\epsilon\mathrm{S}\epsilon\tau)=\nabla a-\nabla^2\mathrm{P},$$

or

$$2\dot{\epsilon}-4\epsilon^2=\nabla a-\nabla^2\mathrm{P};$$

therefore

$$2\dot{\epsilon}=\mathrm{V}\nabla a, \text{ and } 4\epsilon^2=\nabla^2\mathrm{P}-\mathrm{S}\nabla a.. \quad . \qquad (3)$$

The first of these equations gives the required condition; if the forces acting are conservative, $\mathrm{V}\nabla a=0$, and ϵ must be constant in direction and magnitude, the magnitude and pressure being connected by the second equation. The case here considered is the general case of the possibility of a quantity of dead water accompanying a moving solid, and includes that of fluids in relative rest upon or within the earth.

Considering the possibility of a fluid interior of the earth, it must be observed that, owing to precession and nutation, the axis of the earth is not constant in direction, and that, therefore, the condition is not truly satisfied. If, however, the shape of the earth gives a stable form for the fluid, the viscosity of the fluid will tend to mitigate any departure from the apparent rigidity after such motion has once been established.

Precession must also prevent the absolute rest of fluid contained in a vessel upon the earth's surface; and it is possible, though highly improbable, that in this way precession might be demonstrated as Foucault's pendulum demonstrates the earth's rotation.

XXIV. *On the Addition and Multiplication of Logical Relatives.* By JOSEPH JOHN MURPHY, F.G.S. Communicated by the Rev. ROBERT HARLEY, F.R.S.

Read January 25th, 1881.

IN every science, and most of all perhaps in Logic, it is desirable to begin at the beginning. In order the more effectually to do so, I shall avail myself of the method introduced, I believe, by De Morgan, of working in an arbitrarily limited universe.

The simplest possible case is that of a universe containing only two individuals, exactly alike but with different names, A and X. Each of these is the logical negative of the other: whatever is not A is X, and conversely. Let A have also the names B, C, &c., and let X have also the names Y, Z, &c. We call the names of individuals and classes absolute terms. Let the relation between identical names be indicated by 1, and that between contrary names by -1. Both of these relatives are *invertible*; that is to say, each is its own reciprocal, and, if used as a multiplier, may be transposed to the other side of an equation without change. Thus,

if $A = 1B$, then $B = 1A$,

and

if $A = -1X$, then $X = -1A$.

But, as we shall see, in another equally important respect their properties differ.

These two relatives form the following four syllogistic combinations :—

$$A = 1B, \quad B = 1C, \quad A = 1C;$$

$$A = -1X, \quad X = 1Y, \quad A = -1Y;$$

$$A = 1B, \quad B = -1X, \quad A = -1X;$$

$$A = -1X, \quad X = -1B, \quad A = 1B.$$

These syllogisms may, however, be more compendiously expressed by means of canonical equations, using the relative terms only, thus :—

$$1 \times 1 = 1; \qquad (-1) \times 1 = -1;$$

$$1 \times (-1) = -1; \qquad (-1) \times -1 = 1.$$

These equations are also true in common algebra. Their logical interpretations are :—

Identical of identical is identical;

Identical of negative is negative.

Negative of identical is negative;

Negative of negative is identical.

Thus 1 is equal to its own second power, indicating that identity is a *transitive* relation; -1 is not equal to its own second power, indicating that the relation of the logical negative is *intransitive*.

In investigating this simplest possible case, we have now considered the formulæ of conversion and syllogism, which are generally regarded as coextensive with the whole of elementary logic. But there is in the logic of relatives a third operation, which appears to be related to addition as syllogism is to multiplication *. The syllogistic formulæ

* My logical reading has been by no means extensive; and I am quite prepared to find that my ideas have been anticipated; but, so far as I know, what I have written on the addition of relatives is original.

given above show how syllogism is analogous to multiplication: if the relative terms are numerical coefficients, the process is multiplication; if they are logical relatives, it is syllogism. The problem of syllogism may be thus stated:—Given the relations of two terms to a third, to find the resultant relation of the first two to each other. The problem of what I propose to call the addition of relatives is this:—Given two relations between two terms, to find their resultant.

In the case before us the solution is as follows:—

$$1+(-1)=0.$$

This is true both in common algebra and in logic; its logical interpretation is that these two relations cannot coexist; and it is the expression of the law of contradiction within the limits of the present case.

As a further illustration of the relation between the addition and the multiplication of logical relatives, let L signify the relation of teacher and M that of brother; then

$$\mathrm{A}=(L+M)\mathrm{B}$$

means that A is a teacher *and* a brother of B; while

$$\mathrm{A}=(L\times M)\mathrm{B}$$

means that A is a teacher *of* a brother of B.

The order of addition is a matter of indifference, that is to say

$$L+M=M+L,$$

whatever the meaning assigned to L and M. This, as we shall see, is not generally true of the multiplication of

relatives. But the most important law of the addition of relatives is that

$$L+L=L,$$

whatever relative L may be. The same is true of the addition of absolute terms in the logical systems of Jevons, MacColl, and Pierce.

So far as I see, we have exhausted the subject of the formal relations between the terms in the simplest possible universe. The possible interpretations, however, are not confined to those given above; the relative 1 may mean agreement in possessing or not possessing any character whatever, and -1 the corresponding difference. Let the universe, for instance, be a village of one street; let A, B, C, &c. be the houses on one side of the street, and X, Y, Z, &c. the houses on the other side; let 1 be the relation between any two houses on the same side, and -1 the relation between any two houses on opposite sides; then the interpretation of the four syllogistic forms will be :—

Same side with same side is same side;

Same side with opposite is opposite.

Opposite of same side is opposite;

Opposite of opposite is same side.

Now let us imitate the order of nature, and evolve the more complex out of the simpler. The first step we have to take in the direction of greater complexity consists in presenting a case exactly similar to the last, except that the sides of the street which constitutes our universe are distinguished as north and south. The relations now arising cannot be expressed by numerical coefficients; so let us use L (L being the symbol used by De Morgan for relation in general) to indicate the relation of any house

on the north side to any house on the south side, and L^{-1} for the inverse relation *.

In the system here expounded the ru.e for logical conversion is simply to transpose the relative term, changing the sign of its index. This is also valid in common algebra. When a relative is equal to its own reciprocal, it is not necessary to change the sign of the index, though to do so would not be erroneous.

Two other relations arise out of L and L^{-1}. The following syllogistic reasoning is valid in common algebra for all values of L :—

$$\begin{aligned} \mathrm{A} &= L\mathrm{X}, \\ \mathrm{B} &= L\mathrm{X} \textit{ or } \mathrm{X} = L^{-1}\mathrm{B}, \\ \mathrm{A} &= L \times L^{-1}\mathrm{B}, \\ &= L^{\circ}\mathrm{B}; \end{aligned}$$

and it is also valid in logic, with this interpretation :—

A is north of X ;
B is north of X ;
A is fellow-northern of B.

The following is equally valid :—

$$\begin{aligned} \mathrm{X} &= L^{-1}\mathrm{A}, \\ y &= L^{-1}\mathrm{A} \text{ or } \mathrm{A} = Ly, \\ \mathrm{X} &= L^{-1} \times Ly, \\ &= (L^{-1})^{\circ}y, \end{aligned}$$

* The use of the negative index to signify inverse relation is, I believe, the only mathematical expression introduced by De Morgan into logic. The use of the zero index was first, so far as I am aware, introduced into logic in my paper "On an Extension of the ordinary Logic, connecting it with the Logic of Relatives," in the 'Memoirs of the Manchester Literary and Philosophical Society,' Session 1879–80 (*suprà*, pp. 90–101).

with the interpretation

X is south of A,
Y is south of A,
X is fellow-southern of Y.

And, generally, if any two terms stand in any relation L to a third term, they stand in the relation L° to each other.

The canonical equations of the two foregoing syllogisms are the following :—

$$L \times L^{-1} = L^{\circ};$$

and

$$L^{-1} \times L = (L^{-1})^{\circ}.$$

We here see that logical multiplication is not necessarily commutative; that is to say, the product may, as in this case, be changed by changing the order of the factors.

In logic, as in common algebra, every term with zero index is transitive,

$$(L^{\circ})^{2} = L^{\circ},$$

and invertible,

$$(L^{\circ})^{-1} = L^{\circ},$$

properties which are combined in no other terms.

Every relative term with zero index signifies identity or exact similarity in some respect. Equality means exact similarity of magnitude; and the axiom that "equals of equals are equal" is a particular case of the more general truth expressed by

$$(L^{\circ})^{2} = L^{\circ},$$

where L means any relation whatever.

From the four relatives

L *or* north, L° *or* fellow-northern,
L^{-1} *or* south, $(L^{-1})^{\circ}$ *or* fellow-southern,

we obtain sixteen syllogisms, as set forth in the following table, which is a multiplication table, and is arranged as such. (In what follows we shall speak of logical premises as factors and logical conclusions as products.) The multipliers, corresponding to the minors of the ordinary logic, occupy the left-hand column; the multiplicands, corresponding to the majors of the ordinary logic, occupy the top line; any product is found on the line with the multiplier and under the multiplicand *. It will be observed that in half the places the product is zero; this indicates that in the universe and with the relatives under consideration such products do not exist. In the other eight places the products are what we may call affirmative results.

	L	L°	$(L^{-1})^{\circ}$	L^{-1}
L	1 0	2 0	3 L	4 L°
L°	5 L	6 L°	7 0	8 0
$(L^{-1})^{\circ}$	9 0	10 0	11 $(L^{-1})^{\circ}$	12 L^{-1}
L^{-1}	13 $(L^{-1})^{\circ}$	14 L^{-1}	15 0	16 0

The interpretations of these syllogisms are the following †; A, B, and C being houses on the north side of the

* We owe to De Morgan the improvement of writing the minor premise of a syllogism before instead of after the major.

† This table is the same as that given on p. 45 of Prof. Pierce's "Notation

street, X, Y, and Z houses on the south side, and no others existing in the universe under consideration :—

1. A is north of X ;	X is north of W ;	There is no W.
2. A is north of X ;	X is fellow-northern of W ;	There is no W.
3. A is north of X ;	X is fellow-southern of Y ;	A is north of Y.
4. A is north of X ;	X is south of B ;	A is fellow-northern of B.
5. A is fellow-northern of B ;	B is north of X ;	A is north of X.
6. A is fellow-northern of B ;	B is fellow-northern of C ;	A is fellow-northern of C.
7. A is fellow-northern of B ;	B is fellow-southern of W ;	There is no W.
8. A is fellow-northern of B ;	B is south of W ;	There is no W.
9. X is fellow-southern of Y ;	Y is north of W ;	There is no W.
10. X is fellow-southern of Y ;	Y is fellow-northern of W ;	There is no W.
11. X is fellow-southern of Y ;	Y is fellow-southern of Z ;	X is fellow-southern of Z.
12. X is fellow-southern of Y ;	Y is south of A ;	X is south of A.
13. X is south of A ;	A is north of Y ;	X is fellow-southern of Y.
14. X is south of A ;	A is fellow-northern of B ;	X is south of B.
15. X is south of A ;	A is fellow-southern of W ;	There is no W.
16. X is south of A ;	A is south of W ;	There is no W.

In the eight cases where the results are affirmative, they are true of all relatives whatever; their truth is independent of the meaning assigned to L; and they are also

for the Logic of Relatives" (Memoirs of the American Academy, vol. ix.), but with a different notation. He calls these elementary relatives; but I have shown that there is a yet simpler case.

true in arithmetic for all numerical values of L. In the eight remaining cases the results depend on the properties of the relative L and the constitution of the universe. When our logical universe is the actual universe and of indefinite extent, and when the relative is neither transitive nor invertible—that is to say, when neither of the equations

$$L^2 = L \text{ and } L^{-1} = L$$

is true—the eight syllogisms which here have zero products become inconclusive; that is to say, none of the products are of the same form with any of the factors. In arithmetic there is nothing analogous to an inconclusive syllogism, because every number is a factor of every other number.

Let us now suppose the universe to be of indefinite extent, and the relative L, and of course also its reciprocal L^{-1}, to be transitive, the table will then be as follows. It will be seen that two of the places for products are blank; this indicates that the syllogisms are inconclusive.

	L	L°	$(L^{-1})^{\circ}$	L^{-1}
L	1 L	2 L°	3 L	4 L°
L°	5 L	6 L°	7	8 L°
$(L^{-1})^{\circ}$	9 $(L^{-1})^{\circ}$	10	11 $(L^{-1})^{\circ}$	12 L^{-1}
L^{-1}	13 $(L^{-1})^{\circ}$	14 L^{-1}	15 $(L^{-1})^{\circ}$	16 L^{-1}

These syllogisms are true if we assign any transitive meaning whatever to L, such as greater, smaller, above, below, before, or after. Let us, for instance, convene that the cause of a cause is a cause; and let L mean cause,

then L^{-1} will mean effect, L° concause (to use a good though obsolete word), and $(L^{-1})^{\circ}$ coeffect, and all the syllogisms in the foregoing table will be true.

But its most important application is to that part of the ordinary logic which deals with the relation of inclusion*. Using L as the symbol for inclusion, the old "syllogism in Barbara,"

A is B,
B is C,
A is C,

becomes

$$\begin{aligned} \mathrm{A} &= L\mathrm{B}, \\ \mathrm{B} &= L\mathrm{C}, \\ \mathrm{A} &= L^2\mathrm{C}, \\ &= L\mathrm{C}, \end{aligned}$$

or, in language,

A is included in B,
B is included in C,
A is included in C,

of which syllogism the canonical equation is

$$L^2 = L;$$

which we may express in language by saying that the enclosure of an enclosure is an enclosure. And, conversely,

$$(L^{-1})^2 = L^{-1};$$

or the includent of an includent is an includent.

L° in this notation means coenclosure, or enclosure in

* The problems of the ordinary logic are generally stated as dealing with the relations of inclusion and exclusion; but they may easily be generalized so as to deal with coexistence and non-coexistence.

the same includent; and $(L^{-1})^{\circ}$ means coincludent, or includence of the same enclosure. When A and B are coincludents, the old logic would say merely "some A is B;" but these expressions are not equivalent. As we shall see further on, I propose for "some A is B" to say "A and B are participants of each other." Every pair of coincludents are participants; and every pair of participants are coincludents; but we say that coincludents are coincludents of the common part or enclosure, and participants are participants of each other. It must also be remembered that every relative with zero index is used transitively, so that when we speak of coinclusion or coincludence we mean, throughout, inclusion in the same includent, or includence of the same enclosure.

With L meaning inclusion, the interpretations of these sixteen canonical equations are as follows:—

1. Enclosure of enclosure is enclosure.
2. Enclosure of coenclosure is coenclosure.
3. Enclosure of coincludent is enclosure.
4. Enclosure of includent is coenclosure.

5. Coenclosure of enclosure is enclosure.
6. Coenclosure of coenclosure is coenclosure.
7. *Coenclosure of coincludent constitutes no relation.*
8. Coenclosure of includent is coenclosure.

9. Coincludent of enclosure is coincludent.
10. *Coincludent of coenclosure constitutes no relation.*
11. Coincludent of coincludent is coincludent.
12. Coincludent of includent is includent.

13. Includent of enclosure is coincludent.
14. Includent of coenclosure is includent.
15. Includent of coincludent is coincludent.
16. Includent of includent is includent.

When we interpret L as inclusion, there is a double relation between L and L^{-1}: they are not only inverse or reciprocal to each other, but also contrapositive. The contrapositive of the relation between any two terms is defined as the relation between the negatives of those terms. Thus, writing $\bar{a}$ and $\bar{b}$ for the logical negatives of A and B (that is to say, whatever is not A, and whatever is not B), if any one of the following four propositions is true, the rest are true:—

$$\mathrm{A}=L\mathrm{B}; \qquad \mathrm{B}=L^{-1}\mathrm{A};$$
$$\bar{\mathrm{a}}=L^{-1}\bar{\mathrm{b}}; \qquad \bar{\mathrm{b}}=L\bar{\mathrm{a}}.$$

We now go on to that part of the old logic which deals with the relation of exclusion.

When A is not B, I propose to say that A and B are *excludents* of each other; and to use N as the symbol of exclusion.

$$\text{If } \mathrm{A}=N\mathrm{B}, \text{ then } \mathrm{B}=N\mathrm{A};$$

that is to say, N is an invertible relative, or

$$N^{-1}=N.$$

But it is intransitive, or not equal to its own second power —excludent of excludent is not excludent.

Let us use M as the symbol of the contrapositive relation to N, so that if either of the following two propositions is true the other is true:—

$$\mathrm{A}=N\mathrm{B}; \ \bar{\mathrm{a}}=M\bar{\mathrm{b}}.$$

That is to say, if nothing is both A and B, then every thing is either not-A or not-B. In other words, if A and B are

excludents, then not-A and not-B are *alternatives.* Similarly, the truth of either of the following implies the truth of the other :—

$$\mathrm{A} = M\mathrm{B}\,;\ \bar{\mathrm{a}} = N\bar{\mathrm{b}}.$$

That is to say, if every thing is either A or B, then nothing is both not-A and not-B ; and conversely *.

M, like *N,* is invertible and is not transitive. Every invertible relative has this property, that its second power is equal to its zero power. This follows from the definition that when two terms stand in the same relation to a third, they stand to each other in the zero power of the same relative. Thus

$$\begin{aligned} &\mathrm{A} = N\mathrm{B}, \\ &\mathrm{B} = N\mathrm{C}, \\ &\mathrm{A} = N^2\mathrm{C} = N^\circ\mathrm{C}, \end{aligned}$$

whereof the canonical equation is

$$N^2 = N^\circ\,;$$

and similarly

$$M^2 = M^\circ.$$

That is to say,

Excludent of excludent is coexcludent.
Alternative of alternative is coalternative.

This combination of properties—equal to its own reciprocal and not equal to its own second power—exists in negative unity and in no other number.

* The introduction of this relation, which I call alternation, into logic is due to De Morgan. What I call an alternative he calls a complement.

We have now these four relations :—

$$A = LB, \quad \textit{or } \text{A is included in B};$$
$$A = L^{-1}B, \quad \textit{or } \text{A includes B};$$
$$A = NB, \quad \textit{or } \text{Nothing is both A and B};$$
$$A = MB, \quad \textit{or } \text{Every thing is either A or B.}$$

These are expressed in Boole's and Jevons's systems by the following :—

$$A = AB, \text{ or } A\bar{b} = 0$$
$$AB = B, \text{ or } \bar{a}B = 0;$$
$$A = A\bar{b}, \text{ or } AB = 0;$$
$$\bar{a} = \bar{a}B, \text{ or } \bar{a}\bar{b} = 0.$$

If we multiply these four relatives into each other, exchanging the places of N and M in the column of multipliers, we obtain the symmetrical result shown in the following Table. The products $\bar{l}$ and $\bar{l}^{-1}$ ought properly to be omitted from this table, and the squares containing them left blank, because these products are not of the same form with any of the factors; but they are inserted in order to facilitate comparison with the larger table further on. As we shall see, $\bar{l}$ is the denial of L, and $\bar{l}^{-1}$ of L^{-1}; that is to say,

$$A = \bar{l}B \quad \text{means some A is not B,}$$

and

$$A = \bar{l}^{-1}B \text{ means some B is not A.}$$

	L	L^{-1}	N	M
L	L	L°	N	
L^{-1}	$(L^{-1})^{\circ}$	L^{-1}	$\bar{l}$	M
M	M		L^{-1}	M°
N	$\bar{l}^{-1}$	N	N°	L

We have next to consider the addition of these relatives. In what follows, U means coextension with the universe, and U^{-1} membership of the universe wherewith the other absolute term is coextensive; 1, as before, means identity, and -1 the logical negative, or the relation between A and whatever is not A. Six pairs from the four terms are added together, as follows:—

1. $L+L^{-1}=1.$	2. $N+M=-1.$
3. $N+L=0.$	4. $N+L^{-1}=0.$
5. $M+L=U^{-1}.$	6. $M+L^{-1}=U.$

These canonical equations may be called syllogisms, because they give the resultant of two propositions; but they are not syllogisms in the technical sense, because they do not eliminate any middle term. Their interpretations are as follows:—

1. A is included in B;
   A includes B;
   A and B are identical.

2. Nothing is both A and B;
   Every thing is either A or B;
   A and B are the negatives of each other.

3. Nothing is both A and B;
   A is included in B;
   One or both of the premises must be untrue.

4. Nothing is both A and B;
   A includes B;
   One or both of the premises must be untrue.

5. Every thing is either A or B;
   A is included in B;
   A is a member of the universe wherewith B is coextensive.

6. Every thing is either A or B;
   A includes B;
   A is coextensive with the universe whereof B is a member.

We return to the multiplication of relatives. If we multiply these four relatives by the logical negative, and inversely, we get the following eight canonical equations, where it will be seen that the inverse order of multiplication gives the contrapositive result.

1. $(-1)\times L \; = M.$	5. $L \times(-1)=N.$
2. $(-1)\times L^{-1}=N.$	6. $L^{-1}\times(-1)=M.$
3. $(-1)\times N \; = L^{-1}.$	7. $N \times(-1)=L.$
4. $(-1)\times M \; = L.$	8. $M \times(-1)=L^{-1}.$

That is to say:—

1. Negative of enclosure is alternative.
2. Negative of includent is excludent.
3. Negative of excludent is includent.
4. Negative of alternative is enclosure.

5. Enclosure of negative is excludent.
6. Includent of negative is alternative.
7. Excludent of negative is enclosure.
8. Alternative of negative is includent.

We have seen that in two important properties N and M are analogous to negative unity. If we call -1, N, and M negative terms, and L and L^{-1} positive ones, we shall see that the foregoing equations conform to the rule that like signs by multiplication produce $+$, and unlike signs $-$.

By adding these four relatives to unity and to negative unity, we obtain the eight following canonical equations. It will be remembered that, as 1 is the symbol of identity, the following equation is true,

$$A = 1A,$$

whatever be the meaning of A.

1. $1+L \quad = 1.$	5. $-1+L \quad = 0.$
2. $1+L^{-1} = 1.$	6. $-1+L^{-1} = 0.$
3. $1+N \quad = 0.$	7. $-1+N \quad = -1.$
4. $1+M \quad = U+U^{-1}.$	8. $-1+M \quad = -1.$

These are to be interpreted as follows, in syllogistic form :—

1. A is identical with B;
   A is included in B;
   A and B are identical.

2. A is identical with B;
   A includes B;
   A and B are identical.

3. A is identical with B;
   Nothing is both A and B;
   One or both of the premises must be untrue.

4. A is identical with B;
   Every thing is either A or B;
   Every thing is both A and B.

5. A and B are the negatives of each other;
   A is included in B;
   One or both of the premises must be untrue.

6. A and B are the negatives of each other;
   A includes B;
   One or both of the premises must be untrue.

7. A and B are the negatives of each other;
Nothing is both A and B;
A and B are the negatives of each other.

8. A and B are the negatives of each other;
Every thing is either A or B;
A and B are the negatives of each other.

It will be observed that if we add L or L^{-1} to 1, the sum is 1, and if we add N or M to -1, the sum is -1. As L and L^{-1} are positive and N and M negative, this may be compared with the equation in the logic of absolute terms in the systems of Jevons, MacColl, and Pierce,

$$A + AB = A;$$

that is to say, if we add a part to the whole we do not increase the whole. This is expressed by the equation already given,

$$L + L = L.$$

It will be observed also that of the above eight equations, the sum of three is zero, of two unity, of two negative unity, and of one

$$U + U^{-1},$$

indicating that the A and the B of the equation are each of them a member of a universe wherewith the other is coextensive; in other words, both A and B are coextensive with the universe. To give an instance of the reasoning :—

Inertia and gravity are coextensive.
There is nothing which has neither inertia nor gravity.
Every thing has both inertia and gravity.

These eight equations work out less symmetrically than we might have expected; but their asymmetry may, perhaps, point to some principle which I do not now see.

Every syllogism has its reciprocal. This is found by substituting for the factors their reciprocals, and writing them in reversed order. Thus, if L and M be any two relatives, the syllogism

$$L \times M$$

has for its reciprocal

$$M^{-1} \times L^{-1};$$

that is to say

$$(L \times M)^{-1} = M^{-1} \times L^{-1}.$$

This is also true in arithmetic; but in arithmetic we do not need to reverse the position of the factors, as this has no effect.

Every relative term has its corresponding denial; and these we propose to write as follows:—

L,	or enclosure,	is denied by	$\bar{l}$,	or indifferent;
L^{-1},	or includent,	,,	$\bar{l}^{-1}$,	or indeterminant;
N,	or excludent,	,,	$\bar{n}$,	or participant;
M,	or alternative,	,,	$\bar{m}$,	or inessential*.

The following is a fuller statement of the same:—

1. A $= L$B; *or* A is B.

Denied by

2. A $= \bar{l}$B; *or* some A is not B.

* These verbal expressions are, I think, more self-explaining than De Morgan's equivalent ones.

3. $A=L^{-1}B$; *or* B is A.

Denied by

4. $A=\bar{l}^{-1}B$; *or* some B is not A.

5. $A=NB$; *or* no A is B; *or* no B is A.

Denied by

6. $A=\bar{n}B$; *or* some A is B; *or* some B is A.

7. $A=MB$; *or* every thing is either A or B.

Denied by

8. $A=\bar{m}B$; *or* something is neither A nor B.

Each of these eight relatives has its form with zero index, making altogether sixteen terms, which combine into 256 syllogisms; but of these only 76 are conclusive. They are set forth in the following table. As before, the multipliers are in the left-hand column, and the multiplicands in the top line. The terms are so arranged, that if the table be folded by the middle either vertically or horizontally, each of the four terms

$$L \qquad L^{-1} \qquad N \qquad M$$

will rest on the term denying it; and each of the four terms

$$L^\circ \qquad (L^{-1})^\circ \qquad N^\circ \qquad M^\circ$$

will rest respectively on

$$\bar{l}^\circ \qquad (\bar{l}^{-1})^\circ \qquad \bar{n}^\circ \qquad \bar{m}^\circ.$$

The numbers in the top left-hand corner of each square are those of the multiplier and the multiplicand which form the product in that square; and the numbers in the bottom right-hand corner are those of the multiplier and

<table>
<tr><th></th><th>1. Enclosure. L.</th><th>2. Coenclosure. L°.</th><th>3. Coincludent. $(L^{-1})^\circ$.</th><th>4. Includent. L^{-1}.</th><th>5. Excludent. N.</th><th>6. Coexcludent. N°.</th><th>7. Coalternative. M°.</th><th>8. Alternative. M.</th><th>9. Inessential. $\bar{m}$.</th><th>10. Coinessential. $\bar{m}^\circ$.</th><th>11. Coparticipant. $\bar{n}^\circ$.</th><th>12. Participant. $\bar{n}$.</th><th>13. Indeterminant. $\bar{l}^{-1}$.</th><th>14. Coindeterminant. $(\bar{l}^{-1})^\circ$.</th><th>15. Coindifferent. $\bar{l}^\circ$.</th><th>16. Indifferent. $\bar{l}$.</th><th></th></tr>
<tr><td>1. Enclosure. L.</td><td>1|1
L
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L°
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L°
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N
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N°
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8|4</td><td>1|9
$\bar{m}$
9|4</td><td>1|10
$\bar{m}^\circ$
10|4</td><td>1|11
11|4</td><td>1|12
12|4</td><td>1|13
$\bar{l}^{-1}$
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$(\bar{l}^{-1})^\circ$
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15|4</td><td>1|16
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<tr><td>2. Coenclosure. L°.</td><td>2|1
L
4|2</td><td>2|2
L°
2|2</td><td>2|3
3|2</td><td>2|4
L°
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5|2</td><td>2|6
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12|2</td><td>2|13
16|2</td><td>2|14
14|2</td><td>2|15
15|2</td><td>2|16
13|2</td><td>L°, Coenclosure. 2.</td></tr>
<tr><td>3. Coincludent. $(L^{-1})^\circ$.</td><td>3|1
$(L^{-1})^\circ$
4|1</td><td>3|2
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$(L^{-1})^\circ$
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L^{-1}
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$\bar{l}$
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$\bar{n}^\circ$
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16|3</td><td>3|14
14|3</td><td>3|15
15|3</td><td>3|16
13|3</td><td>$(L^{-1})^\circ$, Coincludent. 3.</td></tr>
<tr><td>4. Includent. L^{-1}.</td><td>4|1
$(L^{-1})^\circ$
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L^{-1}
2|1</td><td>4|3
$(L^{-1})^\circ$
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L^{-1}
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$\bar{l}$
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6|1</td><td>4|7
M°
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M
8|1</td><td>4|9
9|1</td><td>4|10
10|1</td><td>4|11
$\bar{n}^\circ$
11|1</td><td>4|12
$\bar{n}$
12|1</td><td>4|13
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14|1</td><td>4|15
$\bar{l}^\circ$
15|1</td><td>4|16
$\bar{l}$
13|1</td><td>L^{-1}, Includent. 4.</td></tr>
<tr><td>5. Excludent. N.</td><td>5|1
$\bar{l}^{-1}$
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2|5</td><td>5|3
$\bar{l}^{-1}$
3|5</td><td>5|4
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N°
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N
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8|5</td><td>5|9
9|5</td><td>5|10
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11|5</td><td>5|12
$\bar{l}^{-1}$
12|5</td><td>5|13
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<tr><td>6. Coexcludent. N°</td><td>6|1
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3|6</td><td>6|4
N°
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N°
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13|6</td><td>N°, Coexcludent. 6.</td></tr>
<tr><td>7. Coalternative. M°.</td><td>7|1
M°
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3|7</td><td>7|4
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M°
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M
8|7</td><td>7|9
9|7</td><td>7|10
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15|7</td><td>7|16
13|7</td><td>M°, Coalternative. 7.</td></tr>
<tr><td>8. Alternative. M.</td><td>8|1
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L^{-1}
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6|8</td><td>8|7
M
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M°
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$\bar{l}$
9|8</td><td>8|10
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<tr><td>9. Inessential. $\bar{m}$.</td><td>9|1
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$\bar{l}^{-1}$
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$\bar{m}^\circ$
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13|9</td><td>$\bar{m}$, Inessential. 9.</td></tr>
<tr><td>10. Coinessential. $\bar{m}^\circ$.</td><td>10|1
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$\bar{m}$
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$\bar{m}^\circ$
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13|10</td><td>$\bar{m}^\circ$, Coinessential. 10.</td></tr>
<tr><td>11. Coparticipant. $\bar{n}^\circ$.</td><td>11|1
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$\bar{n}^\circ$
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$\bar{n}$
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13|11</td><td>$\bar{n}^\circ$, Coparticipant. 11.</td></tr>
<tr><td>12. Participant. $\bar{n}$.</td><td>12|1
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$\bar{n}^\circ$
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$\bar{l}$
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13|12</td><td>$\bar{n}$, Participant. 12.</td></tr>
<tr><td>13. Indeterminant. $\bar{l}^{-1}$.</td><td>13|1
$\bar{l}^{-1}$
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$\bar{l}^{-1}$
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$(\bar{l}^{-1})^\circ$
13|16</td><td>$\bar{l}^{-1}$, Indeterminant. 13.</td></tr>
<tr><td>14. Coindeterminant. $(\bar{l}^{-1})^\circ$.</td><td>14|1
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$(\bar{l}^{-1})^\circ$
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13|14</td><td>$(\bar{l}^{-1})^\circ$, Coindeterminant. 14.</td></tr>
<tr><td>15. Coindifferent. $\bar{l}^\circ$.</td><td>15|1
$\bar{l}^\circ$
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$\bar{l}^\circ$
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$\bar{l}$
13|15</td><td>$\bar{l}^\circ$, Coindifferent. 15.</td></tr>
<tr><td>16. Indifferent. $\bar{l}$.</td><td>16|1
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$\bar{n}$
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12|13</td><td>16|13
$\bar{l}^\circ$
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$\bar{l}$
14|13</td><td>16|15
15|13</td><td>16|16
13|13</td><td>$\bar{l}$, Indifferent. 16.</td></tr>
<tr><td></td><td>L, Enclosure. 1.</td><td>L°, Coenclosure. 2.</td><td>$(L^{-1})^\circ$, Coincludent. 3.</td><td>L^{-1}, Includent. 4.</td><td>N, Excludent. 5.</td><td>N°, Coexcludent. 6.</td><td>M°, Coalternative. 7.</td><td>M, Alternative. 8.</td><td>$\bar{m}$, Inessential. 9.</td><td>$\bar{m}^\circ$, Coinessential. 10.</td><td>$\bar{n}^\circ$, Coparticipant. 11.</td><td>$\bar{n}$, Participant. 12.</td><td>$\bar{l}^{-1}$, Indeterminant. 13.</td><td>$(\bar{l}^{-1})^\circ$, Coindeterminant. 14.</td><td>$\bar{l}^\circ$, Coindifferent. 15.</td><td>$\bar{l}$, Indifferent. 16.</td><td></td></tr>
</table>

the multiplicand of the reciprocal syllogism. Of course, when any syllogism is inconclusive, its reciprocal is inconclusive, and conversely. The vacant squares, as before, indicate inconclusive syllogisms.

It will be seen that the entire table is divided into sixteen squares of sixteen syllogisms each. The top left-hand square is the same as that on page 209. The bottom right-hand square is the same as that on page 207, except that the syllogisms which there have zero products are here inconclusive.

In interpreting those syllogisms which contain factors with zero index, it must be remembered that when we deal with inclusion and coinclusion, exclusion and co-exclusion, &c., the unexpressed middle term is understood to be always the same; that is to say, it is the same thing which is included or excluded. For instance, the syllogism

$$N \times N^\circ = N.$$

A and B are excludents of each other,
B and C are coexcludents of A,
A and C are excludents of each other.

The analogy of all terms with zero index to unity fails without this convention. But we adhere to this only as between a term and its own zero power, not as between one term and the zero power of another. This will explain the only unsymmetrical or anomalous-looking results in the table. We have seen (page 211) that coincludents $[(L^{-1})^\circ]$ of any third term are participants $[\bar{n}]$ of each other. Consequently

$$(L^{-1})^\circ \times \bar{n} = \bar{n}^\circ, \text{ and } \bar{n} \times (L^{-1})^\circ = n^\circ;$$

that is to say, coincludent of participant is coparticipant, and conversely. But the syllogisms

$$L^{-1} \times \overline{n}^{\circ} \text{ and } \overline{n}^{\circ} \times L^{-1}$$

are inconclusive, because the factors, having different letters, are not understood as referring to the same unexpressed middle term.

Considered exclusively with respect to their logical form, there are four classes of relatives. All of them have representatives in this table.

1. Every relative of the first class is transitive, or equal to its own second power—and invertible, or equal to its own reciprocal. The only numerical coefficient which unites these two properties is unity. To this class belong all terms with zero index.

2. Every relative of the second class is transitive, but not invertible. The only numerical coefficients which unite these two properties are zero and its reciprocal infinity. To this class belong L and L^{-1}.

3. Every relative of the third class is not transitive, but is invertible. The only numerical coefficient which unites these two properties is negative unity. To this class belong N and M, with their denials $\overline{n}$ and $\overline{m}$.

4. Every relative of the fourth class is neither transitive nor invertible. These properties are united in all numerical coefficients whatever except unity, zero, infinity, and negative unity. To this class belong $\overline{l}$ and $\overline{l}^{-1}$.

The denial of a relative of the first class belongs to the third class (*e. g.* "equal" is denied by "unequal").

The denial of a relative of the second class belongs either to the second class (*e. g.* "greater than" is denied by "no greater than") or to the fourth (*e. g.* "enclosure" or "A is B" is denied by "indifferent" or "some A is not B").

The denial of a relative of the third class belongs either to the first class (*e. g.* "unequal" is denied by "equal") or to the third (*e. g.* "excludent" or "no A is B" is denied by "participant" or "some A is B").

The denial of a relative of the fourth class belongs either to the second class (*e. g.* "indifferent" is denied by "enclosure," as above) or to the fourth class (*e. g.* "teacher" is denied by "not teacher").

Of our sixteen terms, the old logic recognizes only four, namely

L or inclusion,	N or exclusion,
$\bar{n}$ or partial inclusion,	$\bar{l}$ or partial exclusion;

and these are respectively equivalent to the well-known forms of proposition

A,	E,
I,	O.

All our syllogisms are in the fourth figure, having the minor premise first, and the middle term second in the minor and first in the major.

Of course there is an endless number of relations belonging to each of our four classes. Logicians, however, are right in treating inclusion and exclusion as the fundamental relations of the science. Inclusion and exclusion (L and N) belong respectively to the second and third classes of relatives; and it is worth while to remark that the two fundamental relations of geometry, namely direction and distance, belong to the same. That is to say, direction is transitive but not invertible. The following syllogism is valid :—

A is north of B ;
B is north of C ;
A is north of C.

But if A is north of B, B is not north of A. Distance, on the contrary, is invertible but not transitive : if A is a mile from B, B is a mile from A ; but from the premises

A is a mile from B,
B is a mile from C,

we can only infer that A and C are equidistant by a mile from B. Perhaps these relations may hereafter lead to the establishment of some connexion between logic and geometry analogous to that which Boole and his continuators have shown to exist between logic and arithmetical algebra.

Note added while correcting proof.—On seeing the abstract of this paper, Prof. Pierce wrote to me that my addition of relative terms is, in all but notation, the same as his internal multiplication of the same. This is quite true. See his paper in the 'Algebra and Logic,' reprinted from the 'American Journal of Mathematics,' vol. iii.

XXV. *On the Growth and Use of a Symbolical Language.*

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Read March 22nd, 1881.

IN an article on "Symbolical Reasoning," in a recent number of 'Mind' (No. 17, Jan. 1880), I have described the relation between symbolical reasoning and ordinary verbal reasoning as analogous to that between machine labour and ordinary manual labour. To trace this analogy through all its various points of resemblance would take too long; but there is one point which deserves some notice, as it bears more especially upon the present subject.

For what kinds of operations are machines usually invented? A little reflection will show that one common and prominent characteristic of such operations is *sameness*; we employ machines to perform operations *which have to be frequently repeated, and repeated in the same unvarying manner*. Sewing-machines, knitting-machines, reaping-machines, and, in fact, the great generality of machines, however widely they may differ in other respects, resemble each other in this.

For what kinds of expressions and relations, mathematical or logical, do we usually invent symbols? We shall find, as before, that the common characteristic of such expressions and relations is *sameness*—that they are expressions and relations *which have to be repeated fre-*

quently. When any complex expression or relation is perceived to have a tendency to recur again and again, we economize thought, time, and space if we denote this expression or relation by some simple, suggestive, and easily formed symbol which we may always recognize as doing duty for its more complex equivalent.

The representive symbols thus invented combine afterwards among themselves into new expressions and relations of more or less complexity, and give birth, in their turn, when the necessity or convenience arises, to fresh representative symbols, whose abbreviating power bears, on an average, the same ratio to that of the symbols they displace, as the abbreviating power of the latter bears to that of *their* immediate progenitors. In strict conformity with this law of symbolical growth the science of mathematics has gradually attained its present wonderful power within the limits of its application; and in strict conformity with the same law, the science of logic, which is now evidently entering on quite a new phase of existence, will probably before long, and within much wider limits of application, surpass the achievements of mathematical science itself.

Now it is clear that the power and progress of any symbolical language must depend very largely upon the judgment exercised, first, as to whether, in any proposed case, a new symbol is really required, or would on the whole be useful, and, secondly (supposing the need of a new symbol to be admitted), as to the kind of symbol that should be selected. With regard to the first point, we must remember that the introduction of a fresh symbol is always accompanied by the disadvantage that it adds a fresh item to the load which the memory has to carry, and it is only when its advantages more than outweigh this very serious drawback that it should be admitted as a permanent addition to the existing vocabulary. Can we

discover any general principles or rules which should guide us in this important matter of admission or rejection? Let us examine a few of the symbols which we now possess, and see whether any such rules can be discovered.

The ratio which the circumference of a circle has to its diameter, namely, 3·14159 &c., is one that occurs frequently, and for this reason mathematicians express it by a single arbitrary symbol π. The ratio which the diagonal of a square has to its side, namely, 1·41421 &c., is another ratio which also occurs frequently, and yet mathematicians do *not* express this by any single arbitrary symbol, nor would any mathematician think the introduction of such a symbol desirable. Why is this? The answer is obvious: the latter ratio may be expressed, *without any fresh definition or explanation*, by a very brief and simple combination of existing symbols, namely by the combination $\sqrt{2}$; while we know of no brief and easily formed combination of existing symbols, *requiring no fresh definition*, which would accurately and unambiguously express the former ratio.

From these and other analogous examples we may safely assume as one guiding principle, that some conventional symbol of abbreviation should be used as a substitute for any expression that has a tendency to recur frequently, *provided that no suitable combination of existing symbols* (*i. e.* a combination short, simple, and requiring no fresh definition or explanation) *can be found to replace it.*

The next point is, as a rule, more important and also less easily decided. It is this:—Granting the necessity for some new symbol of abbreviation, what kind of symbol should be selected?

In the case of the symbol π, to which we have already alluded, this question of suitable selection is, it is true, of

secondary importance; almost any arbitrary symbol of easy formation would have done just as well; but this is an exception to the general rule. Consider the symbol a^n, which has been invented as an abbreviation for the product of n equal factors, each equal to a; that is, a^2 for aa, a^3 for aaa, and so on. If the first of these products, namely aa, were the only one that had a tendency to recur, we may be quite sure that mathematicians would remain satisfied with it in its original form, and would never have accepted the innovation a^2 as its equivalent. But since aaa, $aaaa$, &c. have also the same tendency of frequent recurrence, the appropriateness of the symbol selected is evident: the numerical index reminds us of the number of equal factors; and we are at once provided with a more effective notation for considering the properties and relations of all expressions that are products of equal factors, as, for instance, in the binomial theorem.

Let us now examine the *raison d'être* of that remarkable class of symbols which were invented at a more advanced stage of the science (by whom I know not), and which give such a wonderful sweep and power to symbolical language generally, logical as well as mathematical; I refer to that class of symbols of which $f(x)$ may be taken as a specimen. This symbol denotes *any complex expression whatever* (mathematical or logical) that contains the simpler expression x, *in any relation whatever*, as one of its constituents. What was the special need which this symbol was invented to supply?

We have often to consider what an expression would become if one of its constituents were taken away and a fresh constituent put into its place, just as people sometimes speculate as to what would be the effect upon a ministerial policy if a certain member of the cabinet were to resign and a certain other person appointed in his place.

If $f(x)$ denote the expression of which x is a constituent, then $f(a)$ will denote the new expression which is formed by substituting a for x. To take a simple case, let $f(x)$ denote the algebraical expression $\frac{6}{3-x}+x+5$; then $f(2)$ will denote $\frac{6}{3-2}+2+5$, and will be equal to 13; $f(0)$ will denote $\frac{6}{3-0}+0+5$, and will be equal to 7; $f(x-5)$ will denote $\frac{6}{3-(x-5)}+(x-5)+5$, and will be equal to $\frac{6}{8-x}+x$; and so on.

The last symbol $f(x-5)$ warns us of a danger to be carefully guarded against in the introduction of fresh symbols, namely *the danger of ambiguity*. The meaning here attached to it *might* in certain cases be confounded with an older and commoner meaning; for the symbol $f(x-5)$ also denotes *the product of the two factors* f *and* x $-$ 5. How is this danger of ambiguity to be guarded against? We might, it is true, guard against it by adopting, instead of f, a totally new symbol of some unwonted shape; but this is a course to be avoided if possible. Strange-looking symbols somehow offend the eye; and we do not take to them kindly, even when they are of simple and easy formation. Provided we can avoid ambiguity, it is generally better to intrust an old symbol with new duties than to employ the services of a perfect stranger. In the case just considered, and in many analogous cases, the context will be quite sufficient to prevent us from confounding one meaning with another, just as in ordinary discourse we run no risk of confounding the meanings of the word *air* in the two statements—' He assumed an air of authority," and " He resolved the

air into its component gases." In the special case of $f(x-5)$ no ambiguity exists when the letter f is used in no other sense throughout the investigation on which we happen to be engaged; and when it *is* used in another sense, all risk of confusion is obviated by simply employing instead of f, in the expressions $f(x)$, $f(x-5)$, &c., some other symbolic letter, such as ϕ.

We may recapitulate the results so far arrived at thus:—

1. A new symbol, or an old symbol with a new meaning, to be accepted as a permanent addition to the existing stock, should represent an expression or relation that tends to occur frequently, and that cannot be logically expressed by any short combination of existing symbols.

2. Provided we can avoid ambiguity, it is better to employ an old symbol in a new sense than to invent a totally new symbol.

3. The symbol chosen should be of short and easy formation and lead to symbolic expressions of simple and symmetrical forms.

The illustrations hitherto given have been borrowed from mathematics, because these are more familiar to the general reader than any that could have been taken from symbolical logic; yet the latter is in every respect the simpler science of the two,—simpler in the conceptions*

* That the conceptions which underlie the very elements of symbolical mathematics are by no means easy to grasp will be admitted by any one who has attempted to explain to beginners the real logical meaning of the "Rule of Signs" in multiplication and division of ordinary algebra. This difficulty meets the tyro on the very threshold of the science. When he has advanced a few steps further, he is confronted with a symbolical paradox which it has taxed the ingenuity of the subtlest mathematical intellects to explain, and of which no rational explanation whatever was given until within very recent times; I allude to the useful and important yet perplexing symbol $\sqrt{-1}$.

represented by symbols, simpler in the smallness of the number of symbols employed, and simpler in the mechanical operations that have to be performed. In claiming this advantage for logic over mathematics, I speak solely of that scheme of symbolical logic which I, rightly or wrongly, consider the simplest and most effective, namely the scheme which I have explained and illustrated in 'Mind,' in the 'Proceedings' of the London Mathematical Society, in the 'Educational Times,' and in the 'Philosophical Magazine.' According to this scheme the whole and sole duty of the logician is to investigate the relations in which *statements* (*i. e.* assertions and denials) stand towards each other. For all practical reasoning-purposes a *statement* may be defined as anything that conveys directly through a bodily sense (as the eye or ear) any information (true or false) to the mind. In this sense a nod or a shake of the head is a perfectly intelligible statement. The Union-Jack fluttering from the mast of a ship conveys as clear and definite information as the words "This is a British ship" shouted through the captain's speaking-trumpet; and therefore the flag is as much a statement in the logical sense as the words; and, like the words, it may (as we know by experience) be a true or a false statement.

Logic, then, being concerned with statements, the analogy of ordinary algebra suggests the propriety of denoting simple statements by single letters, and the relations in which statements stand to each other either by the relative positions of the statement-letters, or by separate and distinct symbols. Therefore a very important inquiry in laying the foundation of a practical symbolical calculus for solving logical problems is this:—What are the characteristics and relations which most frequently distinguish or connect the statements of

an argument? Foremost among distinctions we shall find that of *truth* and *falsehood.* All intelligible statements may be divided into two great classes, the *true* and the *false.* Every statement must belong to one or other of these two classes, *though we may not always know to which.* If it were not for this element of uncertainty, reasoning would be purposeless, and logic would have no *raison d'être.* This uncertainty (sometimes real, and sometimes only hypothetical) suggests the convenience of dividing the statements of any argument upon which we happen to be engaged into three distinct classes, the *admittedly true,* the *admittedly false,* and the *doubtful.* Borrowing a hint from mathematical probability, we may denote any statement belonging to the first class by the symbol 1, and any statement belonging to the second class by 0, while any doubtful statement (whether the doubt be real or hypothetical) may be denoted by any symbol we choose except these. Now, generally speaking, in the course of any consistent argument or investigation the boundaries of these three classes will be found to be gradually changing; the first two classes, the *admittedly true* and the *admittedly false,* though never encroaching upon each other's ground, will both constantly encroach upon the ground of the third, the *doubtful.*

Statements may also, independently of their truth or falsehood, be divided into two distinct classes, namely *assertions* and *denials.* Every assertion either claims or has already obtained admission into the class denoted by the symbol 1; while its denial contests its right to this symbol, which it claims for itself, and seeks to brand it, as an impostor, with the symbol 0. As long as these two claimants belong to the class of doubtful statements, all that we can say about them is, that the one (either the assertion or its denial) must be true, and the other false.

The denial of any assertion may be conveniently denoted by an accent, thus :—Let x denote the statement, "He is in England;" then x' will denote "He is *not* in England."

The statements hitherto spoken of are simple or elementary statements—that is, statements represented each by a single letter, or a single letter and an accent. Any statement that requires more than one letter to express it may be called a *complex* statement. The principal relations by virtue of which simple statements combine into complex ones are three—namely, *conjunction, disjunction,* and *implication,* corresponding respectively to the three conjunctions *and, or, if.* The first relation is generally symbolized (like multiplication in ordinary algebra) by simple juxtaposition, and occasionally, though never necessarily, by the symbol $\times$; the second (like addition in ordinary algebra) by the symbol $+$; and the third by the symbol $:$, as in the following examples :—

Let x denote the statement "He will go to Paris;" and let y denote the statement "I shall go to York." Then xy denotes the compound statement "He will go to Paris *and* I shall go to York;" the symbol $x+y$ denotes the disjunctive statement "He will go to Paris *or* I shall go to York;" and $x:y$ denotes the implication "*If* he goes to Paris I shall go to York."

A compound statement, as $a\,b\,c$, claims the symbol 1 for *every one* of its factors; a disjunctive statement, as $a+b+c$, claims the symbol 1 for *one at least* of its terms; and the implicational statement $a:b$ claims the symbol 1 for the consequent b, *provided the antecedent* a *is entitled to it,* but neither claims nor disclaims it for b if a is *not* entitled to it.

Brackets are used when necessary to collect the different elements of a complex statement, and so prevent any uncer-

tainty respecting to what complex statement any element or relational symbol belongs. Thus, the compound statement $a(b+c)$, formed by the two *factors* a and $b+c$, is a very different statement from the disjunctive statement $ab+c$, formed by the two *terms* ab and c. So, again, is $(a : b+c)'$, the denial of the whole implicational complex statement $a : b+c$, a very different statement from $a : b+c'$, in which the symbol of denial affects only the element c.

Reciprocal implications—that is, compound implications of the form $(a : b)(b : a)$—occur so frequently that a symbol of abbreviation is convenient. Borrowing again from the existent mathematical stock, we may use* either $::$ or $=$. Thus, either the symbol $a :: b$ or the symbol $a=b$ may be taken as an abbreviation for the reciprocal implication $(a : b)(b : a)$.

The symbol $f(x)$ has been already considered, and is employed in logic in the same sense as in mathematics; that is to say, it denotes any statement whatever that contains x as one of its constituents; but the symbol $f'(x)$, for which no logical meaning analogous to its mathematical one is likely to turn up, may be conveniently employed as an abbreviation for $\{f(x)\}'$.

These are the only symbols that need be employed in the system of symbolical logic which I advocate, and they are amply sufficient not only for the complete solution of any logical problem that I have ever seen solved by any

* To avoid the employment of brackets and repetition as much as possible, it will be convenient to use *both*, with this distinction, that the symbols $:$ and $::$ should be coordinate (*i. e.* of *equal reach* in regard to the statements affected by them), but both subordinate (*i. e.* of *inferior reach*) to the symbol $=$. Thus, $a : b+c :: d+e : f :: g$ is an abbreviation for the complex statement

$$(a : b+c)(b+c :: d+e)(d+e : f)(f :: g),$$

while $a : b+c = d+e : f :: g$ is an abbreviation for

$$(a : b+c) = (d+e : f :: g).$$

other method, but also for the complete solution of many problems which, I think, it would be difficult to solve by any other method with which I am at present acquainted.

The rules which I have proposed for observance in introducing new symbols are, I believe, sound, and I have followed them myself to to the best of my ability. As the science advances, other symbols will, no doubt, become necessary; but they should be introduced slowly, and not till their utility is made clearly manifest.

My statement in 'Mind,' that though $a:b$ implies $a'+b$, it is not equivalent to it, has been called in question, my critics maintaining that there is no real difference between the conditional statement "If a is true, b is true," and the disjunctive statement "Either a is false or b is true." Now, I admit at once that, in the ordinary language of life, disjunctive statements are often made which convey, and are intended to convey, a *conditional meaning*, and, further, that the example which I gave in illustration, namely "He will either discontinue his extravagance, or he will be ruined," is one of them. Many statements, however, are made in common life which are tacitly understood to convey a stronger meaning than logically and literally belongs to them. Take, for instance, the well-known expression, "He will never set the Thames on fire." In its literal sense this very harmless-sounding statement does not commit one to much; it may, with equal safety, be applied to the cleverest man living and to the most incapable idiot. What the practical reasoner would be concerned with in making use of any evidence conveyed to him in such terms would be the *intended* meaning of the speaker; and if his argument should be of such a nature as to necessitate the employment of symbols, the symbol for the statement should denote its *intended* and not its literal meaning. The real

question in dispute is this, does the conditional statement "If a is true b is true," *as I define and symbolize it,* convey a meaning in any way different from the disjunctive statement "Either a is false or b is true," *as I define and symbolize it*?

My argument in 'Mind' was, that since the *denial* of the first, namely "a may be true without b being so," conveys less information than "a is true and b is false," which is the denial of the second, the conditional disjunctive statements of which these are the respective denials cannot be equivalent. As the non-equivalence of the denials, however, is much more evident than that of the affirmative statements, it will be well worth while to give, if possible, a more direct proof of the non-equivalence of the latter.

As it can easily be shown that $a : b$ is equivalent to $1 : a' + b$, the question may be narrowed to this, is the implication $1 : \alpha$, in which α denotes $a' + b$, equivalent to the simple affirmation α? It seems to me that $1 : \alpha$ and α differ in pretty much the same way as the statement "It is well-known that tin is heavier than zinc," and the simpler affirmation "Tin is heavier than zinc;" that is to say, the former implies the latter, but is not implied in it. The statement $1 : \alpha$, in addition to claiming the symbol 1 for itself, asserts that its *protégé* α has fairly made good its right to it; whereas α only claims this symbol on its own account. The symbol $(1 : \alpha)'$, which is the denial of $1 : \alpha$, may be read, "α is not necessarily true;" whereas α', the denial of α, is much stronger, and asserts positively that α is false. It follows from the law of logic called *contraposition* that the denial of the weaker (or implied) statement is stronger than and implies the denial of the stronger (or implying) statement.

The disjunctive statements of ordinary language may be

divided into *conditional* disjunctives and *unconditional* (or pure) disjunctives—the former being those already referred to as conditional in meaning though disjunctive in form. Among these may be classed the famous disjunctive of Edward I., "By God, sir Earl, you shall either go or hang" (symbolically, $g+h$), the meaning of which is evidently, "If you won't go, I will have you hanged." The king, by a not uncommon trick of speech, used the weaker statement in order to express a stronger meaning. The earl, in his reply, "By God, sir king, I shall neither go nor hang" ($g'h'$), secures emphasis of a more direct kind by flatly denying even the king's weak disjunctive, instead of the much stronger conditional statement which would more logically, though less emphatically, express the king's real meaning. The denial of "If you won't go, I will have you hanged," would be the very mild assertion, "I may refuse to go without your having me hanged," a mode of speech which would not at all have suited the temper of Earl Bigod.

As an instance of a simple unconditional disjunctive, we may take the statement, "We shall either go to Brighton or Hastings this summer." There appears to me to be a fundamental difference between the class of disjunctives of which this is a type, and the conditional class of disjunctives previously illustrated. At the same time it must be admitted that in common language, just as statements which are conditional in meaning are often expressed in a disjunctive form, so real disjunctives unconditional in meaning are often expressed in a conditional form. The last example, for instance, "We shall either go to Brighton or to Hastings this summer," might, according to usage and without any perceptible difference of meaning, be expressed as "If we don't go to Brighton this summer, we shall go to Hastings," or in the same words with Brighton and Hastings

interchanged. The fact, however, that conditionals and disjunctives are frequently confounded in ordinary untechnical language, is no reason why they should be so in formal or symbolical logic. Even if I have not succeeded in satisfactorily proving that $a : b$ and $a' + b$ are not synonymous, it is safest, I think, to adopt my view in actual practice. Let it be observed that the hypothesis of non-equivalence commits one to less, and therefore involves less risk to the inferred conclusions. My critics admit with me that $(a : b) : (a' + b)$ is a correct formula; but they would also add the formula $a' + b : (a : b)$, the validity of which I deny. If I am wrong, I am open to the charge of seeking to deprive logic of a new formula which might possibly prove useful, but whose utility has yet to be proved. If my opponents are wrong, they are open to the graver charge of seeking to introduce an erroneous formula, which not only can render no service in reasoning, but might even seriously endanger our conclusions.

As this article is an attempt to explain and illustrate the laws which necessitate the growth, and the principles which determine the form, of a symbolical language, I hope it will not be considered either irrelevant or egotistical, if I give a brief account of the development of my own method. By "my own method" I mean simply the *method which I discovered* (including those features which it has in common with the prior methods of others), as well as those characteristics which are peculiar to itself. What these are I leave to others to decide. The question is certainly irrelevant to the expressed object of this article; and its discussion would only provoke the natural impatience of the reader. I only mention this at all in order to explain that I use the possessive pronoun *my* merely as a convenient abbreviation, and in a sense which cannot possibly give offence to any of my fellow workers.

As I stated in my third paper in the 'Proceedings' of the London Mathematical Society, my method originated in a question in probability proposed in the 'Educational Times' for June 1871*. The question (as I understood it) may be thus generalized:—"Given that the variables x, y, z are each taken at random between given limits, what is the chance that an assigned function of these variables, say $\phi(x, y, z)$, will be real and positive? When I began to solve the problem I found that, in addition to the particular event whose chance was required, I should have to consider the relations in which this event stood towards several other events on which it more or less depended. It struck me that it would help the memory and facilitate the reasoning, if I registered the various events spoken of in regular numerical order in a table of reference. The event whose chance had to be found resolved itself into a concurrence of two distinct but not independent events 1 and 2; and I denoted the *chance* (or probability) of this concurrence by the symbol $p(1 . 2)$. The compound event 1 . 2 implied the occurrence of a third event 3, which it was necessary also to take into account; I had therefore to replace $p(1 . 2)$ by $p(1 . 2 . 3)$. But the consideration of 1 . 2 . 3 could not be separated from the consideration of a fourth event 4, in conjunction with which it might happen, but not necessarily; and the probability of the concurrence 1 . 2 . 3 depended materially upon whether it happened in conjunction with this fourth event or without it. Denoting the *non*-occurrence of this fourth event by the symbol : 4, I thus had the equation $p(1 . 2 . 3) = p(1 . 2 . 3 . 4) = p(1 . 2 . 3 : 4)$. Proceeding in this way, but in a somewhat groping and tentative manner,

* The subject of probability was one which I had recently taken up at the request of Mr. J. C. Miller, the mathematical editor of the 'Educational Times,' who felt great interest in it himself, and strongly recommended it as "an unworked vein in which I should find many treasures."

I finally resolved the problem into a form which brought it within the reach of the integral calculus; in other words, *I had somehow determined the limits of integration*, though I hardly knew how. I applied the same method successfully to two or three other problems in the 'Educational Times,' but without being able to make any material improvement in it. Whilst occupied with these researches, the editor of the 'Educational Times' sent me a very neat and simple geometrical solution by Mr. G. S. Carr of the very problem which had given me so much trouble. This so discouraged me (in the belief that I was only wasting my time) that I threw up the whole subject in disgust, and determined for the future to eschew all mathematics that did not fall within the very narrow limits of my requirements as a teacher.

When, six years afterwards, I broke my resolution and again took up the subject of probability, my mind naturally reverted to the old abandoned method; and it then struck me that, with all its defects, it had one important merit, namely *independence of geometrical diagrams*, and that, consequently, it would be well worth my while to apply myself patiently to the task of removing its defects and developing it, if possible, into something better.

My first step was to drop the letter p (for *probability*), which I thought might, without ambiguity be left understood; so that, for instance, 1.2.3 should replace p(1.2.3) as an abbreviation for "the probability of the event 1.2.3." My next step was to use letters instead of numbers, as ABC instead of 1.2.3, and an accent to denote non-occurrence, as ABC′ instead of 1.2:3. But at this point a difficulty presented itself: how was ABC, the chance of the compound event ABC, to be distinguished from ABC, the product of the chances A, B, C? for the chance of the compound event would not generally be the

product of the chances of the separate events. To guard against this risk of confusion I decided to use capitals, as ABC, when the chance of the whole compound event was meant, and small italic letters, *abc*, when the product of the separate chances *a*, *b*, *c* was meant. Thus, though the chances A, B, C were separately equal to *a*, *b*, *c*, the symbol ABC would not (except in the case of independent events) be equivalent to the symbol *abc*. The equivalence of A(B+C) and AB+AC, and of similar expressions, I discovered before I introduced letters instead of numbers.

So far, I had made no real advance: the substitution of a literal for a numerical notation improved perhaps the *appearance* of the method; but it did not affect it practically. In applying the method to such questions as required the integral calculus it still remained a tentative method; I still groped my way towards conclusions in particular cases without the help of any general rules of procedure. At last I was struck by the fact that the events registered in my tables, and whose chances were denoted by the letters, were all of the form $x>x_1$, $x_2>x$, $x_2>x_1$, $y>y_1$, $y_2>y$, $y_2>y_1$, &c., and *had all reference to the limits of the different variables.* This suggested the idea of a partial return to the original numerical notation and classifying the events according to the variable spoken of. I denoted the event and also the chance of the event $x>x_1$ by x_1, the event $x_1>x$ and its chance by $x_{1'}$, and so on for x_2, $x_{2'}$, x_3, $x_{3'}$, y_1, $y_{1'}$, &c. This was a very important step so far as my method related to integration limits; and after this its development in this direction was comparatively rapid—too much so for me to remember very accurately its different stages. Still, I looked upon the method as essentially and inseparably connected with probability; and even when I had decided that it would be more convenient and less confusing to let my symbols de-

note *logical statements* rather than *mathematical chances,* I could not for some time turn to any account the independence of mathematics which I had thus secured for the method. The notion of the mutual exclusiveness of events (or statements) connected by the sign + clung to the method up to a very late period; in fact, I was in the very act of writing my first article "On Symbolical Reasoning" for the 'Educational Times,' when the needlessness of this restriction occurred to me. I had written down my definitions of the equations $ABC=1$ and $ABC=0$ in the following words:—

The equation $ABC=1$ asserts that all the three statements are true; the equation $ABC=0$ asserts that all the three statements are *not* true, *i. e.* that at least *one* of the three is false;

and I had to consider suitable definitions of the equations $A+B+C=1$ and $A+B+C=0$. It was quite evident that the equation $A+B+C=0$, whether the statements A, B, C were mutually exclusive or not, must assert that *all the three statements are false*; and *the very words used* in the previous definitions of $ABC=1$ and $ABC=0$ suggested that, as a symmetrical complement of this, the equation $A+B+C=1$ should assert that all the three statements are *not* false, *i. e.* that at least *one* of the three is true. The only question to decide was whether the rule of multiplication, $(A+B)(C+D)=AC+AD+BC+BD$, would still hold good. A very little consideration showed that it would; so, though the method was correct, so far as it went, on either supposition, I judged it wiser to leave room for possible future development by adopting the wider rather than the narrower hypothesis for its basis.

Finding myself thus, at the end of my investigation, on logical instead of mathematical ground, I naturally began to study the relation in which my method stood towards

the ordinary logic, and especially towards the syllogism. The only book on logic that I possessed was Prof. Bain's work; and to this I turned. The resemblance which my method bore to Boole's, as therein described, of course struck me at once; but Boole's treatment of the syllogism was more likely to put me on the wrong track than to help me. As my most elementary symbols denoted *statements,* not necessarily connected with quantity at all, I could not see how the syllogism, with its ever recurring *all, some, none,* could be brought within the reach of my method. The Cartesian system of analytical geometry at last supplied the desiderated hint as to the proper mode of procedure. In this system, as every mathematician knows, *one single point* is spoken of in every equation, but with the understanding that it is a *representative* point, and that the equational statement made respecting it is also true respecting every other point in the locus expressed by this equational statement.

The symbol :, which I had already begun to use as an occasionally convenient abbreviation for the word "implies," now became almost imperative. Syllogistic reasoning is strictly restricted to *classification.* The statement "All X is Y" is equivalent to the conditional statement "If any thing belongs to the class X, it must also belong to the class Y." Speaking then of something originally unclassed, if x denote the statement "It belongs to the class X," and if y denote the statement "It belongs to the class Y," then the implicational statement $x : y$ (or x implies y) will be equivalent to the syllogistic statement "All X is Y."

It was evident after this that $x : y'$ would be the proper symbolical expression for "No X is Y;" but, strange to say, the discovery of the suitable symbolic expressions for "Some X is Y" and "some X is not Y" caused me no

small trouble, even though I had previously more than once wondered under what circumstances the symbol $(x:y)'$ would be required. For a long time I did not recognize this $(x:y)'$ as the equivalent (in classication) of "Some X is not Y," and $(x:y)'$ as the equivalent of "Some X is Y." In my second communication to the Mathematical Society I used the symbol $v:xy$ to denote "Some X is Y;" and it was only when I had read the very just objection made by one of the referees to my introduction of the arbitary and possibly non-existent class V that it suddenly flashed upon me that the true symbolical expression for "Some X is Y" should be $(x:y')'$, the denial of the implication $x:y'$, and that the true symbolical expression for "Some X is not Y" should be $(x:y)'$, the denial of $x:y$.

The next new symbol which I introduced into my symbolic system was the symbol x_a, to express the *chance of* x *being true on the assumption that* a *is true.* The circumstances which suggested this symbol to me are curious and instructive. My first idea was to use the symbol x_c to denote the chance of x being true, the suffix c being merely suggestive of the word *chance* and not denoting a statement. In fact, this was the notation which originally formed the basis of my fourth paper, "On the Calculus of Equivalent Statements," when it was first communicated to the London Mathematical Society. While this paper was in the hands of the referees, I was occupied with a problem proposed to me by Mr. C. J. Monro, and involving among other things the consideration of a chance $(xz)_c$, which I at first considered as equal to $x_c(x:z)_c$, being under the idea that, since $x:z$ expressed the conditional statement "If x is true z is true," $(x:z)_c$ would be the proper symbol to express the *chance* that if x is true z is true. On reflexion I discovered that this,

plausible as it sounded, would lead to inconsistency of notation. For, since $x:z$ is equivalent to $z':x'$, consistency of notation required that $(x:z)$ should denote the same chance as $(z':x')_c$; and, *as I had interpreted the symbols*, this would not be the case. The chance that z is true, on the assumption that x is true, is *not* generally equal to the chance that x is false on the assumption that z is false. I was thus forced to the conclusion that I had put a wrong interpretation on the symbols $(x:z)_c$ and $(z':x')_c$, which *must* be equivalent, and that neither of them, therefore, was the proper symbol for the chance which I wished to express. It became necessary, therefore, since there did not appear to be sufficient data for logically *inferring* a correct expression for this chance, to invent a new and arbitrary symbol for it; and then the important question presented itself as to what that symbol should be. It must, if possible, be brief and easily formed; it must be formed, at least partly, of the symbols x and z; and yet it must be some *unambiguous* combination of those symbols—that is to say, a combination which should convey no other meaning either by definition or by implication. Out of several symbols that offered themselves as candidates for the important post to be filled, I at last selected the symbol z_x as the one most likely to perform effectively the duties required of it.

The symbol z_x being thus fairly installed, I was struck by the resemblance between it in some respects and the symbol z_c. Both expressed the *chance* of the truth of z, though on generally different assumptions; and, what was more remarkable, some of the formulæ which I had obtained involving the constant suffix c, as

$$(\alpha+\beta)_c=\alpha_c+\beta_c-(\alpha\beta)_c,$$

were also true when for c I substituted the variable suffix

x. This suggested the propriety of considering c too as a *statement,* instead of a mere arbitrary abbreviation for "the chance of the truth of," and it soon became evident what that statement must be. The constant suffix c, like the variable suffix x, must denote *a statement taken for granted;* but, unlike the variable x, it must denote a statement whose truth is taken for granted *always*—that is, *throughout the whole of an investigation.* In other words, the suffix c must be *an exact equivalent for the logical symbol* 1.

This, however, necessitated other symbolical changes. As long as the *suffix* c did not denote a *statement,* I was at liberty to use this letter in conjunction with the letters a and b in *other positions* as a *statement;* so that c_c (like a_c and b_c) would simply denote the chance of the truth of the statement c, and its value might vary from 0 to 1; but with the new meaning of the suffix c, we should always have $c_c = 1$. It thus became expedient to leave c at liberty to discharge other functions in company with its old comrades a and b, and to intrust the duty of denoting universally admitted statements to some letter whose services in other capacities could be more easily spared. I decided, after some hesitation, on the Greek letter ϵ, which is easily formed, pleasing to the eye, and not often wanted. It may be asked, why was I not satisfied with the symbol 1, which already denoted an admitted statement? My answer is, first, that I thought this numeral would not *look well* in frequent companionship with *literal* suffixes; and, next, that I thought it better to reserve it, in company with other numerals, for distinguishing statements of the same class or series, as a_1, a_2, a_3 &c., which, though different statements, will generally be found to have some common factor or characteristic a.

Having thus decided that ϵ should denote a statement

of acknowledged truth, that x_a should denote the chance of the truth of x on the assumption that a is true, and that therefore x_ϵ must simply denote the chance that x is true, *with no assumption beyond the understood data of the problem,* it soon became evident that this notation would express many of the laws of probability in neat and compact formulæ, and also that it would contribute towards precision of reasoning from its constant reference, by means of its suffixes, to the assumptions on which any argument in probability rested. It is well known that of all mathematical subjects probability is the one in which mistakes are most apt to be made; and these mistakes are usually the result of correct reasoning based upon unperceived false assumptions. These assumptions, for the most part, would be readily seen to be false, if they were only *expressed*; a notation therefore that actually forces them on the attention must be considered as possessing one very important advantage in that fact alone.

As this new scheme of probability-notation quite superseded that which formed the basis of the paper which had been already submitted to the referees of the Mathematical Society, these gentlemen naturally declined (on the scheme being communicated to them through the Honorary Secretary, Mr. Tucker) to pronounce any opinion either upon the original paper or on the proposed alterations, till the whole was recast and rewritten. When this was done, and the paper again submitted to them, they advised its publication.

In my former paper in 'Mind,' "On Symbolical Reasoning," I referred to the analogy between the relation connecting *antecedent* and *consequent* in logic and that connecting *subject* and *predicate* in grammar. Would it be presumptuous to suggest as a probable hypothesis that this analogy is more than a mere coincidence, and that it

really points to an original identity? It does not seem unreasonable to suppose that in the very early stage of human speech, each separate word represented a *complete statement* and conveyed its own independent information. On this supposition, the growth of vocal language would proceed according to laws in some respects analogous to those which shape the development of a language of symbols. Our abstract nouns, for instance, seem to be nothing but abbreviations for original statements. Take the compact and well-known saying, "Unity is strength." What is this but an abbreviation for the conditional statement, "If a company be *united,* they will be *strong*"? or, as it may be otherwise expressed, "If the statement symbolized by the abbreviation *unity* for 'They are united' be applicable to a company of persons, so will also the statement symbolized by the abbreviation *strength* for 'They are strong.'"

But here I must stop. Speculations as to the primæval forms of human speech do not come fairly within the limits prescribed by the title of this article; and further discussion of the subject in this direction would therefore be irrelevant.

XXVI. *On a Chemical Investigation of Japanese Laquor, or Urushi.* By Mr. SADAMA ISHIMATSU, late of Tokio University. Communicated by Professor ROSCOE, Ph.D., F.R.S.

Read February 18th, 1879.

THE Japanese have a peculiar juice of a plant with which they manufacture their beautiful cabinets and boxes so celebrated all over the world, that, now, those articles varnished in imitation of those which are finished with this juice are termed japanned articles. However, as far as I am aware of, no attempt has yet been made to analyze the substance, and none seem to know what it is.

During a few months last year I ventured to undertake the examination of this body, so as to afford some clue to those who will have the opportunity of examining this substance in future more fully and perfectly with ample time.

Urushi and its Cultivation.*

Japanese laquor is nothing but a sap of a certain kind of tree called *Rhus vernicifera,* and commonly termed laquor-tree, and growing chiefly between the North latitudes 23° and 38°. The plantation of this tree constitutes one of the most important parts in the agriculture of certain districts of the Empire, and in all the laquor-producing

* This description is partly derived from a popular account of the urushi manufacture published for the use of Japanese primary schools. A similar statement has been already translated into French by M. Ory, and published in 1875 by the French Asiatic Society.

provinces was protected in the time of feudal government, just as the production of tea and mulberry-tree are protected now by the local government assisted by special officers for this purpose.

As to the mode of planting, there are two different ways :—In the first case, some time at the end of autumn or beginning of winter the seeds from grown trees are collected and ground in a mortar called "*usu*," by means of a "*rine*," which plays the same office as the pestle to the mortar; then they are well stirred and washed in the solution of caustic lye which they obtain, in a way much the same thing as lixiviation, from ash of common wood. They are next put into a rice-straw bag and soaked under water, or, still better, under the urine of horses, until the next spring, when, at or about the 2nd of May, the mass is taken out of the water or urine, and then well spread over the previously well tilled and prepared ground specially for this purpose. Then in time the small plants will germinate.

That of the second method is conducted by digging the roots of old trees; several pieces of roots are cut off by means of a knife; and these are then directly put under ground until the germs come out, and are afterwards transferred to any desired place. The process is much simpler; but it is said that the trees obtained from this source are much more liable to wither or die out from external causes than the first one; therefore it is always preferable to adopt the first method where it is in any way practicable.

After the plants have attained sufficient height, they are usually planted in the hill-sides, corners of farms, plains, on river-banks, or on any other vacant places where the land-tax is light. Urushi-tree or laquor-tree is chiefly cultivated in the provinces Yamato, Dewa, Oshu, Yechigo, Yechizen, Kai, Shiushū, &c.

In about four to five years the tree grows large enough

to be tapped without any injury to the tree itself; and after three or four years more, in the districts where from its seeds the manufacture of candles is not carried out, it is cut down, and from the entire tree the whole of the "urushi" is extracted; hence we never meet with any large trees in these districts. In the provinces of Aidsu and Youezawa, where the manufacture of candles from its seeds is the principal production, the trees themselves are never cut, and cutting them was prohibited by the authorities in feudal times, so that even nowadays many trees are as high as 30–35 feet.

How to obtain the Juice "Urushi."

The tapping of the juice is conducted from the end of April to the end of November. For this purpose a number of incisions are made in the bark, which just reach the wood; the sap immediately runs out, which, coming in contact with air, is blackened on the surface, and forms in time a hard crust.

These incisions are made at first about 36 centimetres distant from one another, on the alternate sides of the trunk; and the sap is collected by means of a bamboo or iron spatula.

After about four days new incisions are made above and below the former cuttings, and the sap is collected by a spatula as before. Similar operations are conducted until the end of the due season, when the whole tree is covered with a number of cuttings, and (in the districts where only sap is obtained and candles are not manufactured from its seeds) is cut down. The branches from trees which are cut down are made into pieces of about 2·5 feet, and made into fagots, and soaked under water from ten to twenty days; they are then taken out, and the incisions are made by means of a certain kind of knife, and the juice is collected. This is the way by which juice is

obtained from branches; and the juice obtained in this way becomes very hard on drying, being mostly used for priming, and is known as "*shesime-urushi.*"

The quality of urushi depends upon the season in which it is tapped and also on the condition of climate and nature of soil, as well as the care taken for its cultivation—the juice that is tapped just at the beginning of the season and towards the last being of much inferior quality; that just obtained at the middle is considered the best. The raw product is a slightly sticky liquid having a dirty grey colour, and always covered with a black crust where it comes in contact with the atmosphere.

This juice is then put into a large tub, where it is allowed to stand for some time; then, in time, finer and better quality and inferior and worse ones separate out into two layers. It is then separated by means of decantation. The superior quality is stirred in the open air in sunlight, as you may see in some parts of the city of Tokio, for the purpose of allowing a certain excess of water to evaporate, after which it assumes a brilliant dark brown or nearly black colour; but in thin layers it is almost transparent. The further operations which the juice undergoes before it is ready for use are as follows:—It is first of all filtered through a strong porous paper called "*yoshino gami,*" and then mixed with the kind of colouring-matter with which it is desired to tint the juice.

It is a great pity that white cannot be laquored with the juice; but it is said that in the province of Noto, at a place called "Washima," white is laquored; but the process is kept unknown, and is rather doubtful.

Various trees produce more or less the same kind of sap. There is one kind of tree called hazē or hāgi, belonging also to the *Rhus* family, which has almost exactly the same appearance as laquor-tree; and one cannot distinguish the

two at a glance. This tree grows more largely in warmer climates than in cold; therefore this tree abounds in the south of the Empire, being one of the great agricultural products, candles being manufactured from its seeds like from those of laquor. Laquor-tree, on the contrary, is mostly found in colder climates.

These conditions made me suppose that the laquor-tree and hagē or hāgi tree were originally one and the same tree, but changed somewhat in property by change of climate, soil, or cultivation. We have many instances of plants being changed in character by the change of place. It is the actual case in Japan that a certain kind of tree called "*yōudsu,*" which in warm climates produces a fruit which has a very pleasant fragrance, when carried up to the north is changed to another kind of tree called "*gédsu,*" which is in all its outward appearance exactly similar to *youdsu,* but produces an entirely different kind of fruit. A plant called "*nasu,*" which is, I think, the same thing as the English egg-plant, produces in the south of Japan a fruit which is quite long, but when carried to the north produces only round-oval-shaped fruit. It is also the case that a cane-sugar plant which grows in the West Indies, when brought to America forms no seed which is capable of producing another plant. Again, hagē or hāgi produces also a poisonous gas, the effect of which is exactly the same as laquor, but rather less in power; and, furthermore, this tree produces a small quantity of laquor, but the quantity is too small to be profitably extracted.

Chemical Investigation.

During a few months I have had the opportunity of examining roughly into the nature of "urushi" in the laboratory of Tokio University.

The specimen of "urushi" which I have examined was

obtained from Kuyemon Nakamuraya, in Tokio, a large urushi-keeper.

It is a milky juice of a pale grey colour; and the Japanese call it "*ash-colour*," from its colour resembling so much that of ash. It gives out a certain kind of volatile acid, poisonous in its property, and some persons are seriously attacked by it, producing great swellings on the face especially, and even the whole body where the acid comes in contact. During my examination in the laboratory, one day one of the apparatus-keepers came in and was violently attacked by it, producing ugly swellings all over his face. He told me at the time it was exceedingly itchy, and by using the solution of acetate of lead, chloride of potash, and carbonate of soda, was said to have recovered from this suffering within a week.

The poison that is evolved from urushi acts only on certain persons. I had to work with it for many days, yet never had any attack of the kind nor felt any uneasiness by it.

Urushi being heavier than water, sinks to the bottom; and under this condition oxidation does not take place; so the colour remains unaltered, and the mass remains soft as long as it may be kept in this way.

It has a sweetish characteristic smell and has an irritating taste. It burns with a very luminous flame, evolving dense black smoke like oil of turpentine. It is soluble in absolute alcohol, ether, benzol, &c. to a great extent, leaving behind a blackish grey residue, in which gum was found.

Urushi, on exposure to the atmosphere, rapidly loses its weight, and at the same time blackens on its surface, forming in time a hard crust—although this loss is different in different specimens, varying in the specimens I have examined from 25 to 35 per cent.

When the laquor is exposed to sunlight in an atmosphere of carbonic acid in hermetically closed flasks, the blacking does not take place; and, to my surprise, I found a great deal of moisture collected on the sides of the flask. The loss of weight in the air is almost, if not entirely due to the escape of water with a minute quantity of carbonic acid, which may be formed by the oxidation of some organic compound existing in the laquor. The attempt has been made to estimate the relative amount of carbonic acid and water, yet it was not successful at the time, being too difficult, and it must be left open to future investigation.

The laquor is a substance which is very difficult to dry; and the way by which the Japanese artists dry the laquored articles is this:—Those who perform these operations have a square wooden box of various sizes according to the amount of work they do, the insides of which are furnished with shelves to hold the laquored articles; and the boxes are provided with doors. In order to do this, the inside is moistened with spray of water, and then laquored articles are introduced and the door closed. It is a well known fact to the artist that the removal of air, dry air, or heating it are the great checks to the drying of the laquor. It is usually the case to dry a paste like gum arabic or dextrin, to place it in a current of air, dry air, or heat it; but in the case of the laquor the reverse is the case. This seems strange, but it is really the fact. I have inquired of many artists, they all say the same. This is then true, as we cannot deny the fact; and there must be some reason or other for it being so.

According to my opinion, the following is the probable explanation of it, if not really the true one. If we expose the laquored articles in the current of air, dry air, or heat it, then only the surface is dried and forms a crust—a wall as it were; and this impervious crust prevents the volatile

matter, as water &c., to escape; it is thus prevented from drying any further. However, in case of moist air, the drying takes place much more slowly, so the volatile matters which are contained in the interior have sufficient time to escape, and the complete drying takes place. In winter the laquor dries with much more difficulty than in summer time; and in the "rainy season" especially it dries very quickly, probably due to the dampness of the atmosphere for the above reason. It is also said that "urushi" dries much quicker by the addition of a small quantity of alcohol or camphor.

Fused with caustic potash just at the temperature at which potash fuses, then treated with water and filtered, on addition of a little dilute H_2SO_4 to neutralize the alkali no precipitate was obtained.

The blackening of the laquor in the air is by many supposed to be due to the combined action of light and air; but this was proved to be erroneous. First, I made a square box which has a well-fitted sliding cover, and the inside of which was made perfectly black, so that no light is admitted to enter; in it a small quantity of fresh laquor on a piece of paper was put in at night in the dark; and on looking the next morning it was observed that the surface of the laquor was covered with a perfectly black wall, proving that it is not due to the light.

Second, the bottle in which I kept my laquor more than three months during my examination was exposed to the incident light of the laboratory; then the surface of the laquor in the bottle turned perfectly black, while those portions which were in contact with the sides of the bottle, which receives as much light as if there were not any glass sides before it, was not at all blackened.

This phenomenon is just complementary to the first one, that the blacking in the atmosphere is, in all probability,

due to the oxygen of the air, but not to the light alone nor to the combined action of air and light (as might have been supposed).

The laquor when distilled with water gives a colourless distillate which is slightly acid to test-paper. The attempt has been undertaken to examine this acid, but not successfully, on account of too minute a quantity of the acid that is evolved.

The distillation by itself and in a current of steam were tried; but the results in both cases were the same as the first one. Then, lastly, distilled with a small quantity of dilute H_2SO_4 into the solution of acetate of lead; but scarcely any precipitate was obtained.

The laquor mixes with any kind of fixed oil in all proportions; hence the oil is often added as an adulteration; but sometimes a very small quantity is added purposely to make the laquor more mobile.

The specimen of urushi which I obtained in my laboratory for examination consisted of the following three substances:—

	I.	II.
Part soluble in absolute alcohol	58·24	58·23
Gum	6·34	6·30
Residue	2·24	2·30
Moisture and other volatile matter	33·18	33·17
	100·0	100·0

As I have mentioned already, the laquor loses its weight rapidly when exposed to the atmosphere: for the determination I weighed out samples each time from a well-stoppered bottle and determined by difference.

Then this was treated with absolute alcohol, and the filtrate evaporated to small bulk, and dried at 100° C. until the weight remains constant. This is put down as "part soluble in absolute alcohol" in the above analysis.

The residue was treated then with hot water, and the filtrate evaporated to dryness, and dried at 100° C., and put down as gum.

The residue after gum has been dissolved out is now dried on a weighed filter, and, after drying at 100° C., weighed and put down as residue.

Moisture and other volatile matters are, of course, determined by the difference.

The examination of the amount of soluble part in alcohol after the laquor has been exposed for some 20 or 30 days in the sunlight, shows that the soluble portion increased up to 72·82 per cent. This lost 25 p. c. water and other volatile matter on exposure, the difference being therefore 58·3 p. c., which is nearly equal to, and practically the same as the analysis given in the preceding page. From this we see that there is no material change in the amount of soluble.

Now a perfectly dried laquor, after being finely powdered and dried at 100° C., was analyzed, and gave the following result :—

Part soluble in alcohol	18·07 per cent.
Gum	3·63 ,,
Residue	78·30 ,,
	100·0

It occurs to me, from this analysis, that the laquor on perfectly drying, alcohol as well as water has greater difficulty to get access to the dried powder than to the undried laquor, although the laquor itself may not have undergone any change.

Now urushi consists of three principal bodies :—a portion soluble in alcohol; gum; and residue. In addition to these, although it contains water and volatile matter, yet they are not strictly constituents.

1. Residue is, I think, nothing more than a mixture of the bark, cellulose, dusts, &c.

2. Gum is soluble in cold as well as hot water. It has no smell, almost no taste, yellowish or rather brownish colour, uncrystalline mass. It is insoluble in alcohol. On subjecting this substance to organic combustion, I got the following percentage amounts of oxygen, hydrogen, and carbon :—

	I.	II.
Carbon	41·20	41·43
Hydrogen	6·51	6·58
Oxygen	52·29	51·99
	100·0	100·0

The formula calculated from I. is $C_{12}H_{22}O_{11}$, and from II. $C_{12}H_{23}O_{11}$. But I think this is near enough to conclude it to be the same substance as ordinary gum.

3. Part soluble in alcohol seems to be the principal part, and has a smell like original laquor, but it never dries up in the ordinary way. It is brownish black, slightly sticky to the touch. When treated with potash solution it forms a bluish-black precipitate; but nothing is obtained on addition of dilute H_2SO_4 to the filtrate.

When boiled with HCl acid it merely forms an elastic mass while hot, something like that when heated sulphur is allowed to drop into cold water.

When boiled with nitric acid, nitrous fumes were given off and the mass gradually became yellow, and finally a beautiful orange-coloured mass was obtained. This mass was washed several times with hot water and then treated with absolute alcohol; the mass was to a greater extent soluble, leaving behind still some quantity of a yellowish mass. (This may be the part that has not been yet sufficiently acted upon by the acid.)

This alcoholic extract is precipitated by either acetate of lead or nitrate of silver as a beautiful yellow mass. I took a quantity of this alcoholic extract and precipitated by means of acetate of lead; and the precipitate was thoroughly washed with absolute alcohol, and then decomposed by dilute sulphuric acid; we cannot decompose the lead-salt with sulphuretted hydrogen (which may be better), as the acid is altogether destroyed by some reducing action of sulphuretted hydrogen or other; and then the acid was again dissolved in absolute alcohol: thus it was separated from the sulphate of lead.

Now this alcoholic solution was again precipitated by means of acetate of lead, and after drying partially in the air-bath, was transferred under the receiver of an air-pump and dried over H_2SO_4. This lead-salt explodes when heated. The amount of lead was estimated by igniting the salt with HNO_3, as PbO; and also the mass was subjected to organic combustion. Nitrogen determined by Dumas's method.

The following numbers were obtained as the result:—

	I.	II.	Mean.
Carbon	26·77	27·10	26·93
Hydrogen	4·10	4·12	4·11
NO_2	18·60	18·28	18·44
PbO	47·41	47·43	47·42
Oxygen	3·12	3·07	3·1
			100·0

The formula calculated from these results is

$$C_{11}H_{20}(NO_2)_2PbO_2.$$

Now, replacing PbO_2 by $(OH)_2$ and $(NO_2)_2$ by H_2, we have

$$C_{11}H_{24}O_2.$$

Now I prepared the silver salt of this substance, and obtained 18·5 per cent. silver, the formula from which does not correspond at all with the above; and therefore silver salt seems to give no help as to the formula for this acid.

As such was the case, I took alcoholic extract of original laquor and precipitated it with acetate of lead, and, after thoroughly washing, dried it at 100°. The lead was estimated as before, and then subjected to organic combustion.

As I had but limited time, only the following two results were experimentally obtained :—

	I.	II.	Mean.
Carbon............	49·84	51·06	50·45
Hydrogen	5·81	5·60	5·705
Oxygen............	40·30	39·84	40·07
PbO	3·50	4·05	3·775
			100·0

Roughly, when a formula is calculated from the above analysis, substituting $(HO)_2$ for PbO_2, we have very nearly $C_{20}H_{30}O_2$.

In concluding my paper, I must say that I do not satisfy myself at all with the analysis, inasmuch as the two different salts do not agree. But I thought it might be interesting to some of you from the fact that, as far as I am aware of, this is the first analysis of the kind attempted.

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1866, Oct. 30. Clifton, Robert Bellamy, M.A., F.R.S., F.R.A.S., Professor of Natural Philosophy, Oxford. *New Museum, Oxford.*

1868, Apr. 28. Darwin, Charles, M.A., F.R.S., F.G.S., F.L.S. *Bromley, Kent.*

1844, Apr. 30. Dumas, Jean Baptiste, Gr. Off. Legion of Honour, For. Mem. R.S., Mem. Imper. Instit. France, &c. 42 *Rue Grenelle, St. Germain, Paris.*

1869, Mar. 9. Frankland, Edward, Ph.D., F.R.S., Prof. of Chemistry in the Royal School of Mines, Mem. Inst. Imp. (Acad. Sci.) Par., &c. *The Yews, Reigate Hill, Reigate.*

1843, Feb. 7. Frisiani, nobile Paolo, Prof., late Astron. at the Observ. of Brera, Milan, Mem. Imper. Roy. Instit. of Lombardy, Milan, and Ital. Soc. Sc. *Milan.*

1853, Apr. 19. Hartnup, John, F.R.A.S. *Observatory, Liverpool.*

1848, Jan. 25. Hind, John Russell, F.R.S., F.R.A.S., Superintendent of the Nautical Almanack.

DATE OF ELECTION.

1866, Jan. 23. Hofmann, A. W., LL.D., Ph.D., F.R.S., F.C.S., Ord. Leg. Hon. Srum Lazar. et Maurit. Ital. Eq., &c. 10 *Dorotheenstrasse, Berlin.*

1869, Jan. 12. Huggins, William, F.R.S., F.R.A.S. *Upper Tulse Hill, Brixton, London, S.W.*

1872, Apr. 30. Huxley, Thomas Henry, LL.D. (Edin.), Ph.D., F.R.S., Professor of Natural History in the Royal School of Mines, South Kensington Museum, F.G.S., F.Z.S., F.L.S., &c. *School of Mines, South Kensington Museum, S.W.*, and 4 *Marlborough-place, Abbey-road, N.W.*

1852, Oct. 16. Kirkman, Rev. Thomas Penyngton, M.A., F.R.S. *Croft Rectory, near Warrington.*

1844, Apr. 30. Owen, Richard, M.D., LL.D., F.R.S., F.L.S., F.G.S., V.P.Z.S., Director of the Nat. Hist. Department, British Museum, Hon. F.R.C.S. Ireland, Hon. M.R.S.E., For. Assoc. Imper. Instit. France, &c. *British Museum, London, W.C.*

1851, Apr. 29. Playfair, Rt. Hon. Lyon, C.B., Ph.D., F.R.S., F.G.S., M.P., F.C.S., &c. 68 *Onslow-gardens, London, S.W.*

1866, Jan. 23. Prestwich, Joseph, F.R.S., F.G.S. *Shoreham, near Sevenoaks.*

1866, Jan. 23. Ramsay, Sir Andrew Crombie, F.R.S., F.G.S., Director of the Geological Survey of Great Britain, Professor of Geology, Royal School of Mines, &c. *Geological Survey Office, Jermyn-street, London, S.W.*

1849, Jan. 23. Rawson, Robert. *Havant, Hants.*

1844, Apr. 30. Sabine, General Sir Edward, R.A., D.C.L., F.R.S., F.R.A.S., Hon. Mem. C.P.S., Chev. of the Prussian Order "*Pour le Mérite.*" 13 *Ashley-place, Westminster, London, S.W.*

1872, Apr. 30. Sachs, Julius, Ph.D. *Prague.*

1869, Dec. 14. Sorby, Henry Clifton, F.R.S., F.G.S., &c. *Broomfield, Sheffield.*

1851, Apr. 29. Stokes, George Gabriel, M.A., D.C.L., Sec. R.S., Lucasian Professor of Mathem. Univ. Cambridge, F.C.P.S., &c. *Lensfield Cottage, Cambridge.*

1861, Jan. 22. Sylvester, James Joseph, M.A., F.R.S., Professor of Mathematics. *Johns Hopkins University, Baltimore.*

DATE OF ELECTION.

1868, Apr. 28. Tait, Peter Guthrie, M.A., F.R.S.E., &c. Professor of Natural Philosophy, Edinburgh.

1851, Apr. 22. Thomson, Sir William, M.A., D.C.L., LL.D., F.R.SS. L. and E., For. Assoc. Imper. Instit. France, Prof. of Nat. Philos. Univ. Glasgow. 2 *College, Glasgow.*

1872, Apr. 30. Trécul, A., Member of the Institute of France. *Paris.*

1868, Apr. 28. Tyndall, John, LL.D., F.R.S., F.C.S., Professor of Natural Philosophy in the Royal Institution and Royal School of Mines. *Royal Institution, London, W.*

CORRESPONDING MEMBERS.

1860, Apr. 17. Ainsworth, Thomas. *Cleator Mills, near Egremont, Whitehaven.*

1861, Jan. 22. Buckland, George, Professor, University College, Toronto. *Toronto.*

1867, Feb. 5. Cialdi, Alessandro, Commander, &c. *Rome.*

1870, Mar. 8. Cockle, The Hon. Sir James, M.A., F.R.S., F.R.A.S., F.C.P.S. *Sandringham-gardens, Ealing.*

1866, Jan. 23. De Caligny, Anatole, Marquis, Corresp. Mem. Acadd. Sc. Turin and Caen, Socc. Agr. Lyons, Sci. Cherbourg, Liége, &c.

1861, Apr. 2. Durand-Fardel, Max, M.D., Chev. of the Legion of Honour, &c. 36 *Rue de Lille, Paris.*

1849, Apr. 17. Girardin, J., Off. Legion of Honour, Corr. Mem. Imper. Instit. France, &c. *Lille.*

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1862, Jan. 7. Lancia di Brolo, Federico, Duc., Inspector of Studies, &c. *Palermo.*

1859, Jan. 25. Le Jolis, Auguste-François, Ph.D., Archiviste perpétuel and late President of the Imper. Soc. Nat. Sc. Cherbourg, &c. *Cherbourg.*

DATE OF ELECTION.

1857, Jan. 27. Lowe, Edward Joseph, F.R.S., F.R.A.S., F.G.S., Mem. Brit. Met. Soc., &c. *Nottingham.*

1862, Jan. 7. Nasmyth, James, C.E., F.R.A.S., &c. *Penshurst, Tunbridge.*

1869, Jan. 12. Saint Venant, Barré de, Ingénieur en chef des Ponts et Chaussées, Corr. Soc. Sci. de Lille et de l'Acad. Romaine des *Nuovi Lincei*, &c.

1867, Feb. 5. Schönfeld, Edward, Ph.D., Director of the Mannheim Observatory.

1834, Jan. 24. Watson, Henry Hough. *Bolton, Lancashire.*

ORDINARY MEMBERS.

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1861, Jan. 22. Alcock, Thomas, M.D., Extr. L.R.C.P. Lond., M.R.C.S. Engl., L.S.A. *Oakfield, Ashton-on-Mersey.*

1873, Jan. 7. Allman, Julius. 2 *Victoria-street.*

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1861, Jan. 22. Anson, Ven. Archd. George Henry Greville, M.A. *Birch Rectory, Rusholme.*

1882, Jan. 24. Arnold, William Thomas, M.A. *Plymouth Grove.*

1837, Aug. 11. Ashton, Thomas. 36 *Charlotte-street.*

1881, Nov. 1. Ashton, Thomas Gair, B.A. 36 *Charlotte-street.*

1874, Nov. 3. Axon, William E. A., M.R.S.L., F.S.I. *Fern Bank,* 1 *Bowker-street, Higher Broughton.*

1865, Nov. 15. Bailey, Charles, F.L.S. *Ashfield, College-road, Whalley Range.*

1824, Jan. 23. Barbour, Robert. 18 *Aytoun-street.*

1876, Nov. 28. Barratt, Walter Edward. *Kersal, Higher Broughton.*

1867, Nov. 12. Barrow, John. 3 *Edensor-place, Dickinson-road, Rusholme.*

1858, Jan. 26. Baxendell, Joseph, F.R.A.S., Corr. Mem. Roy. Phys. Econ. Soc. Königsberg, and Acad. Sc. & Lit. Palermo. 14 *Liverpool-road, Birkdale, Southport.*

DATE OF ELECTION.

1847, Jan. 26. Bazley, Sir Thomas, Bart. *Stow-on-the-Wold, Gloucestershire.*

1877, Nov. 27. Becker, Wilfred, B.A. 4 *Commercial Buildings, Cross-street.*

1878, Nov. 26. Bedson, Peter Philips, D.Sc. *Owens College.*

1847, Jan. 26. Bell, William. 51 *King-street.*

1870, Nov. 15. Bennion, John A., B.A., F.R.A.S. *Barr Hill Mount, Bolton-road, Pendleton.*

1868, Dec. 15. Bickham, Spencer H., jun. *Endsleigh, Alderley Edge.*

1876, Apr. 18. Birley, Thomas Hornby. *Somerville, Pendleton, near Manchester.*

1861, Jan. 22. Bottomley, James, D.Sc., B.A., F.C.S. 7 *Irwell-terrace, Lower Broughton.*

1839, Oct. 29. Bowman, Henry. *Brooklands, Brockham Green, Reigate, London.*

1875, Nov. 16. Boyd, John. 58 *Parsonage-road, Withington.*

1855, Apr. 17. Brockbank, William, F.G.S. *Prince's Chambers,* 26 *Pall Mall.*

1861, Apr. 2. Brogden, Henry, F.G.S. *Hale Lodge, Altrincham.*

1844, Jan. 23. Brooks, William Cunliffe, M.A., M.P. *Bank,* 92 *King-street.*

1860, Jan. 23. Brothers, Alfred, F.R.A.S. 14 *St.-Ann's-square.*

1846, Jan. 27. Browne, Henry, M.D., M.A., M.R.C.S. Engl. *Woodhey, Heaton Mersey.*

1872, Nov. 12. Burghardt, Charles Anthony, Ph.D. 110 *King-street.*

1874, Dec. 15. Carrick, Joseph. *Prince's Chambers,* 26 *Pall Mall.*

1842, Jan. 25. Charlewood, Henry. 1 *St. James's-square.*

1854, Apr. 18. Christie, Richard Copley, M.A., Prof. Hist. Owens College. 2 *St. James's-square.*

1841, Apr. 30. Clay, Charles, M.D., Extr. L.R.C.P. London, L.R.C.S. Edin. 101 *Piccadilly.*

1853, Jan. 25. Cottam, Samuel, F.R.A.S. 6 *Essex-street.*

1859, Jan. 25. Coward, Edward. *Heaton Mersey, near Manchester.*

1861, Nov. 12. Coward, Thomas. *Bowdon.*

1848, Jan. 25. Crowther, Joseph Stretch. *Endsleigh, Alderley Edge.*

1876, Apr. 18. Cunliffe, Robert Ellis. 18 *Vine-grove, Pendleton.*

1861, Apr. 2. Cunningham, William Alexander. *Auchenheath Cottage, Lesmahagow, by Lanark.*

1854, Feb. 7. Dale, John, F.C.S. *Cornbrook Chemical Works, Chester-road.*

1871, Nov. 8. Dale, Richard Samuel, B.A. *Cornbrook Chemical Works, Chester-road.*

DATE OF ELECTION.

1842, Apr. 19. Dancer, John Benjamin, F.R.A.S. 41 *Cross-street.*

1853, Apr. 19. Darbishire, Robert Dukinfield, B.A., F.G.S. 26 *George-street.*

1878, Nov. 26. Davis, Joseph. *Engineer's Office, Lancashire and Yorkshire Railway, Hunt's Bank.*

1869, Nov. 2. Dawkins, William Boyd, M.A., F.R.S. *Museum, Owens College.*

1861, Dec. 10. Deane, William King. 25 *George-street.*

1879, Mar. 18. Dent, Hastings Charles. *City Surveyor's Offices, Town Hall.*

1840, Jan. 21. Gaskell, Rev. William, M.A. 46 *Plymouth Grove.*

1881, Nov. 1. Greg, Arthur. *Eagley, near Bolton.*

1874, Nov. 3. Grimshaw, Harry, F.C.S. *Thornton View, Clayton.*

1875, Feb. 9. Gwyther, R. F., M.A., Lecturer on Mathematics, Owens College. *Owens College.*

1878, Apr. 30. Harland, William Dugdale, F.C.S. 25 *Acomb-street, Greenheys.*

1862, Nov. 4. Hart, Peter. 192 *London-road.*

1839, Jan. 22. Hawkshaw, Sir John, F.R.S., F.G.S., Mem. Inst. C.E. 33 *Great George-street, Westminster, London, S.W.*

1873, Dec. 16. Heelis, James. 71 *Princess-street.*

1828, Oct. 31. Henry, William Charles, M.D., F.R.S. 11 *East-street, Lower Mosley-street.*

1868, Nov. 17. Herford, Rev. Brooke. *Arlington-street Church, Boston, U.S.*

1833, Apr. 26. Heywood, James, F.R.S., F.G.S., F.S.A. 26 *Kensington-Palace-Gardens, London, W.*

1864, Mar. 22. Heywood, Oliver. *Bank, St. Ann's-street.*

1881, Nov. 1. Higgin, Alfred James. 22 *Little Peter-street, Gaythorn.*

1851, Apr. 29. Higgin, James. *Little Peter-street, Gaythorn.*

1846, Jan. 27. Holden, James Platt. 56 *Johnson-street, Smedley Lane, Cheetham Hill.*

1873, Dec. 2. Howorth, Henry H., F.S.A. *Derby House, Eccles.*

1857, Jan. 27. Hunt, Edward, B.A., F.C.S. 42 *Quay-street, Salford.*

1855, Jan. 25. Hurst, Henry Alexander. *The Albany, Liverpool.*

1872, Feb. 6. Jewsbury, Sidney. 39 *Princess-street.*

1870, Nov. 1. Johnson, William H., B.Sc. 26 *Lever-street.*

1878, Nov. 26. Jones, Francis, F.R.S.E., F.C.S. *Grammar School.*

1848, Apr. 18. Joule, Benjamin St. John Baptist. 12 *Wardle-road, Sale.*

DATE OF ELECTION.

1842, Jan. 25. Joule, James Prescott, D.C.L., LL.D., F.R.S., F.C.S., Hon. Mem. C.P.S., and Inst. Eng. Scot., Corr. Mem. Inst. Fr. (Acad. Sc.) Paris, and Roy. Acad. Sc. Turin. 12 *Wardle-road, Sale.*

1852, Jan. 27. Kennedy, John Lawson. 47 *Mosley-street.*
1862, Apr. 29. Knowles, Andrew. *High Bank, Pendlebury.*

1863, Dec. 15. Leake, Robert, M.P. 75 *Princess-street.*
1850, Apr. 30. Leese, Joseph. *Fallowfield.*
1857, Jan. 27. Longridge, Robert Bentink. *Yew-Tree House, Tabley, Knutsford.*
1870, Apr. 19. Lowe, Charles. 43 *Piccadilly.*
1850, Apr. 30. Lund, Edward, F.R.C.S. Eng., L.S.A. 22 *St. John's-street.*
1859, Jan. 25. Lynde, James Gascoigne, M. Inst. C.E., F.G.S. 32 *St. Ann's-street.*

1866, Nov. 13. McDougall, Arthur. *City Flour Mills, Poland-street.*
1859, Jan. 25. Maclure, John William, F.R.G.S. *Cross-street.*
1875, Jan. 26. Mann, John Dixon, Licentiate of the King and Queen's College of Physicians, Ireland; and M.R.C.S. Eng. 16 *St. John-street.*
1879, Dec. 2. Marshall, Alfred Milnes, M.A., D.Sc., Professor of Zoology, Owens College. *Owens College.*
1873, Nov. 4. Marshall, Rev. William, B.A. 81 *High-street, Chorlton-on-Medlock.*
1864, Nov. 1. Mather, William. *Iron Works, Deal-street, Brown-street, Salford.*
1873, Mar. 18. Melvill, James Cosmo, M.A., F.L.S. *Kersal Cottage, Prestwich.*
1879, Dec. 30. Millar, John Bell, B.E., Assistant Lecturer in Engineering, Owens College. *Owens College.*
1881, Oct. 18. Mond, Ludwig. *Winnington Hall, Northwich.*
1877, Nov. 27. Moore, Samuel. 25 *Dover-street, Chorlton-on-Medlock.*
1861, Oct. 29. Morgan, John Edward, M.B., M.A., M.R.C.P. Lond., F.R. Med. and Chir. S. 1 *St. Peter's-square.*

1850, Jan. 24. Newall, Henry. *Hare Hill, Littleborough.*
1873, Mar. 4. Nicholson, Francis, F.Z.S. 62 *Fountain-street.*

1862, Dec. 30. Ogden, Samuel. 10 *Mosley-street West.*
1861, Jan. 22. O'Neill, Charles, F.C.S., Corr. Mem. Ind. Soc. Mulhouse. 72 *Denmark-road.*
1844, Apr. 30. Ormerod, Henry Mere. 5 *Clarence-street.*

DATE OF ELECTION.

1861, Apr. 30. Parlane, James. *Rusholme.*

1876, Nov. 28. Parry, Thomas. *Grafton-place, Ashton-under-Lyne.*

1881, Nov. 29. Peacock, Richard. *Gorton Hall, Manchester.*

1874, Jan. 13. Pennington, Rooke, LL.B., F.G.S. *Mawdsley-street, Bolton.*

1854, Jan. 24. Pochin, Henry Davis. *Barn Elms, Barnes, Surrey, S.W.*

1861, Jan. 22. Radford, William. 1 *Princess-street.*

1854, Feb. 7. Ramsbottom, John. *Fern Hill, Alderley Edge.*

1859, Apr. 19. Ransome, Arthur, M.A., M.D. Cantab., M.R.C.S. 1 *St. Peter's-square.*

1869, Nov. 16. Reynolds, Osborne, M.A., F.R.S., Professor of Engineering, Owens College. *Owens College.*

1880, Mar. 23. Roberts, Lloyd, M.D. *Kersal Towers, Higher Broughton.*

1860, Jan. 24. Roberts, William, M.D., B.A., F.R.S., M.R.C.P. Lond. 89 *Mosley-street.*

1864, Dec. 27. Robinson, John. *Atlas Works, Great Bridgewater-street.*

1822, Jan. 25. Robinson, Samuel. *Blackbrook Cottage, Wilmslow.*

1858, Jan. 26. Roscoe, Henry Enfield, B.A., LL.D. F.R.S., F.C.S., Professor of Chemistry, Owens College. *Owens College.*

1851, Apr. 29. Sandeman, Archibald, M.A. *Tulloch, near Perth.*

1870, Dec. 13. Schorlemmer, Carl, F.R.S., F.C.S. *Owens College.*

1842, Jan. 25. Schunck, Edward, Ph.D., F.R.S., F.C.S. *Oaklands, Kersal.*

1873, Nov. 18. Schuster, Arthur, Ph.D., F.R.S. *Owens College.*

1881, Nov. 29. Schwabe, Edmund Salis, B.A. 41 *George-street.*

1852, Apr. 20. Sidebotham, Joseph, F.R.A.S. *The Beeches, Bowdon.*

1838, Jan. 26. Smith, George Samuel Fereday, M.A., F.G.S. *Grove House, Tunbridge Wells.*

1876, Nov. 28. Smith, James. 35 *Cleveland-road, Crumpsall.*

1844, Apr. 29. Smith, Robert Angus, Ph.D., LL.D., F.R.S., F.C.S., Corr. Mem. Royal Bavarian Soc. 22 *Devonshire-street, All Saints.*

1859, Jan. 25. Sowler, Thomas. 24 *Cannon-street.*

1851, Apr. 29. Spence, Peter, F.C.S., M.S.A. *Alum Works, Newton Heath.*

1870, Nov. 1. Stewart, Balfour, LL.D., F.R.S., Professor of Natural Philosophy, Owens College. *Owens College.*

1863, Oct. 6. Stretton, Bartholomew *Bridgewater-place, High-street.*

DATE OF ELECTION.

1873, Apr. 15. Thomson, William, F.R.S.E., F.C.S. *Royal Institution.*

1860, Apr. 17. Trapp, Samuel Clement. 88 *Mosley-street.*

1879, Dec. 30. Ward, Thomas. *Brookfield House, Northwich.*

1873, Nov. 18. Waters, Arthur William, F.G.S. *Woodbrook, Alderley Edge.*

1874, Dec. 15. Watson, Morrison, M.D., Professor of Descriptive Anatomy, Owens College. *Linda Villa, Heald-road, Bowdon.*

1874, Jan. 27. Watts, John, Ph.D. 23 *Strutt-street.*

1857, Jan. 27. Webb, Thomas George. *Glass Works, Kirby-street, Ancoats.*

1858, Jan. 26. Whitehead, James, M.D., M.R.C.P. Lond., F.R.C.S. Engl., L.S.A., M.R.I.A., Corr. Mem. Soc. Nat. Phil. Dresden, Med. Chir. Soc. Zurich, and Obst. Soc. Edin., Mem. Obst. Soc. Lond. 87 *Mosley-street.*

1839, Jan. 22. Whitworth, Sir Joseph, Bart., F.R.S. *Chorlton-street, Portland-street.*

1859, Jan. 25. Wilde, Henry. *Mill-street, Ancoats.*

1859, Apr. 19. Wilkinson, Thomas Read. *Manchester and Salford Bank, Mosley-street.*

1874, Nov. 3. Williams, William Carleton, F.C.S. *Owens College.*

1851, Apr. 29. Williamson, William Crawford, F.R.S., Professor of Nat. Hist., Anat., and Physiol., Owens College, M.R.C.S. Engl., L.S.A. *Egerton-road, Fallowfield.*

1836, Jan. 22. Wood, William Rayner. *Singleton Lodge, near Manchester.*

1860, Apr. 17. Woolley, George Stephen. 69 *Market-street.*

1863, Nov. 17. Worthington, Samuel Barton, C.E. 12 *York-place, Oxford-street.*

1865, Feb. 21. Worthington, Thomas. 110 *King-street.*

1864, Nov. 1. Wright, William Cort, F.C.S. *Oakfield, Poynton, Cheshire.*

N.B.—Of the above list the following have compounded for their subscriptions, and are therefore Life Members:—

Brogden, Henry.
Johnson, William H., B.Sc.
Sandeman, Archibald, M.A.
Smith, Robert Angus, Ph.D., LL.D., F.R.S.

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